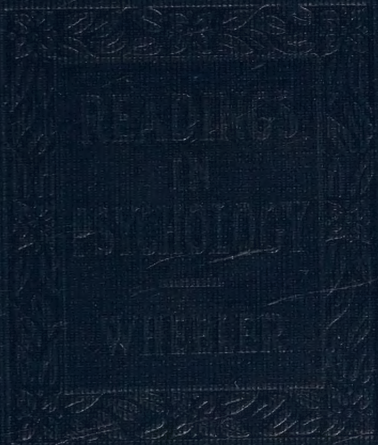

READINGS
IN
PSYCHOLOGY

WHEELER

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WITH SPECIAL READINGS BY

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PREFACE

This volume of Readings has been written for the purpose of giving the beginning student in psychology access to a selected number of experimental investigations. Where classes are large it is impossible for students to make extended use of the periodical and monographic literature, especially when only one copy of an important contribution is available, and it is a burden for the instructor to provide enough references to accommodate every one when suitable contributions are limited in number.

The selection of these Readings has been determined first, by what seems to be the dominant interest in psychology at the present time, second, by their accessibility to the Editor and third, by their relevance to the material presented in *The Science of Psychology*. The organization of the Readings corresponds to the organization of the chapters in the text just mentioned. In order to help the student in understanding the Readings, editorial notes have been written in each instance. These notes are designed to interpret the Readings and to provide continuity throughout the book.

ACKNOWLEDGMENTS

The author takes this opportunity of expressing his appreciation to Professors Harry Helson and Milton Metfessel, and to Dr. Thomas D. Cutsforth, for their contributions written specially for the Readings. Professor Helson has presented an original and critical summary of the outstanding theories of Perception in the light of *Gestalt* Psychology, and has included valuable material not hitherto available in English. Professor Metfessel has brought together the results of his technique in phonophotography. His experiments open a rich field for investigation and promise to accomplish for Audition what configurational experiments have been accomplishing in other fields. Dr. Cutsforth has made intensive studies in the Psychology of the Blind, many of them yet unpublished. The case which he describes was studied while he was a Fellow under the Social Science Research Council, 1928-1929.

Acknowledgment and thanks are due the authors of the several Readings reprinted in this Volume either in whole or in part, not only for permission to use their material, but for their hearty co-operation and interest in the project as a whole. Their names appear in the Table of Contents and in the Text.

Acknowledgment and thanks are due The American Psychological Association, and the Psychological Review Company for permission to use the following material: Readings Two, Sixteen, Seventeen and Eighteen from the *Journal of Experimental Psychology;* Readings Fifteen and Twenty-six from the *Psychological Review;* and Reading Twelve from the *Psychological Monographs.* Thanks are due also the *American Journal of Psychology* for Readings Twenty-two and Twenty-five; the American Psychological Association and the *Journal of Abnormal and Social Psychology* for Readings Three, Four and Nine; the National Committee on Mental Hygiene, and *Mental Hygiene,* for Reading Five; the *British Journal of Psychology* for Reading Twenty-one; the University of Chicago Press and the *American Journal of Sociology* for Reading Twenty-eight; the Williams and Wilkins Company and the *Quarterly Review*

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RAYMOND HOLDER WHEELER.

Lawrence, Kansas,
March, 1930.

GROUP I

SOCIAL BEHAVIOR

READING ONE[1]

THE INDIVIDUAL AND THE GROUP: AN APPLICATION OF EIGHT ORGANISMIC LAWS[2]

Editor's Note

The readings of this book deal with a psychological study of man's behavior, and with the conditions under which that behavior takes place. The most obvious of these conditions, together with their resulting behavior, are chosen first, conditions that pertain to man's social life taken in a scientific sense. This means that the human being will be studied at the outset, just as he is found in a natural, unrestricted environment. Further investigation necessitates restricting the conditions of his behavior, but the instant this restriction is imposed, the resulting behavior becomes limited. It seems expedient to restrict the conditions gradually, thus gradually presenting more and more limited modes of behavior. The less limited the behavior under consideration, the more general it is; the more limited, the more its specialized aspects emerge. The principle adopted, therefore, in arranging the following readings, is a transition from the more general to the more particular and specialized types of material.

This plan satisfies three *desiderata:* (1) It harmonizes with the point of view which has dictated the selection of the readings, namely, the *organismic* view. Here the necessity is emphasized of accounting for the simpler modes of behavior, in man, in terms of the complex—the parts in terms of the whole. If this principle is sound the logical procedure is to consider wholes, first, and the parts, second. (2) A study of the more general concepts of psychology presupposes less technical knowledge and depends to a greater extent upon the common sense information of the reader. As the more specialized material is gradually introduced, the reader is able to make use of the technical vocabulary which he has been acquiring. (3) The same procedure introduces, first, the cruder methods of investigation in psychology, and then leads to a consideration of the more and more refined methods. Thus the reader is first introduced to the easier problems of technique and then, gradually, to the more and more difficult.

Anything imaginable is a part of some whole, and it is related to that whole. The question is, How? As an object of psychological investigation, man is a part of some whole and that whole is the social group in which he lives. How is he related to that whole? To what extent is he dependent upon it, from the standpoint of psychology? This is the problem discussed in the first reading.

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[1] Reading One parallels Chapters II and III, Wheeler, *The Science of Psychology.* Crowell, 1929.

[2] Written for *Readings in Psychology* by Raymond Holder Wheeler, University of Kansas.

Ask a schoolboy, "What constitutes an army?" and he would unquestionably reply, "Soldiers." This is a sensible answer, for if he had ever seen an army, or even a picture of one, the only visible and tangible things about it were men. Ask a military expert, however, to define an army and the answer will be an entirely different one. The emphasis will not be placed upon soldiers, in spite of their importance, but upon *training* and *organization.* The schoolboy did not mention these latter factors, for they are intangible; he probably did not know about them; they cannot be seen; their existence is detectable only as an army is observed in action. But because training and organization are intangible they are none the less real to the military man who understands them. The schoolboy did not see the army as a unit, a whole. He noticed only the parts, and if he saw *all the parts,* the complete whole would not be obvious to him. The military man, however, sees the whole and recognizes that the whole is more than the sum of its parts. An army is more than an aggregate of soldiers; it is an organized group. The "more" that must be considered over and above the parts is precisely the *organization* of the parts within the whole.

It is not easy to apprehend, fully, the significance of the word "organization." When the phrase "organized whole" is used, parts are implied that are in constant relationship to each other and to the whole. Each soldier in the army has certain duties to perform that supplement the duties of all the others, so that in the end a feat can be accomplished, a purpose fulfilled, that without co-operation would be impossible. The army executes a performance which the same number of unorganized men would be incapable of carrying out. The different tasks of different members of the group put each man into a particular relationship to the others and to the army as a unit.

The concept of organization has thus introduced the idea of relationship, a concept not any simpler to understand than organization itself. Relationship signifies a situation that is dynamic in character; it signifies action, or the possibilities of action. The various tasks which each soldier has learned to perform in co-operation with his comrades places him in constant dynamic relationship to them. The relationship to them is that of doing a particular thing at the right time, and it is a dynamic relationship, for it reveals itself in a particular kind of action that depends, for its significance and effect, upon what the other men are doing. His relationship to the whole is also dynamic in that his is an activity that fits properly into the total scheme. His duties mean nothing except as they are defined by the objective of the entire army. The whole is not only more than the sum of its parts, but the activities of the parts are determined by the nature of the whole.

Ask a beginning student in psychology to define a social group and his answer would probably be much like the schoolboy's. The social group would be regarded as nothing more than an aggregation of individuals whether the group be a family, a tribe, a fraternity, a nation, or a race. Individuals would be considered elements or atoms of the group, and the group would be defined in terms of these atoms.

Then, if the student were asked to state how a person came to be an individual he would probably wonder what the question was all about. Naturally, he would think, a person is born an individual; he is a man possessing an innate personality; throughout he is a product of inheritance. It would not occur to the student that possibly a person could no more acquire a personality in the absence of the social group than a man could become a soldier without being in an army. It would be sensible enough for him to assume that an infant, developing in absolute isolation, would acquire all the attributes and properties of human nature: intelligence, skill, morality, self-consciousness and a host of attitudes such as aggressiveness, modesty, acquisitiveness, and shyness. But, like the schoolboy, the beginning student has had no occasion to study the less tangible but none the less real facts of the case. On the other hand the trained psychologist, like the trained military man, sees organization and relationships in the social group that give to the individual the attributes of a human being, just as the army gives to a man the attributes of a soldier.

Consider first any of the qualities of human nature known as traits of character, aggressiveness for example. An aggressive person is one who pushes himself forward in his dealings with others; he assumes leadership in a form of activity in which groups participate; he outwits his business rival; opens and maintains conversations with his companions; leads in a game; suggests things for others to do; and opens the attack in a competitive enterprise. In short, aggressiveness is an attitude *toward others.* It is a dynamic relationship exhibited in the form of behavior, and sustained toward the group in which the individual lives. In the absence of other human beings, to be aggressive would mean nothing. It is a quality quite properly referred to a single human being, but it depends upon a *society*, a whole of which the individual is a part.

Take the trait of modesty. In all of its different meanings—bashfulness, reserve, humility, decency of thought and action—it describes the behavior of one person toward another or toward a group. One is neither modest nor immodest in his behavior toward a tree. The word has no significance except as it applies to a dynamic give-and-take-process between human beings. Again *a society* is the condition that defines the trait; indeed, it is the outstanding source of the trait, for unless a social situation prevailed there would be no quality of human nature known as modesty. Modesty is not something existing inside of human beings; it is a dynamic relationship existing between them; a mode of behavior of one person toward another. Society, composed of human-beings-in-relation, imparts this aspect of human nature to the individual.

In like fashion it would be possible to run the entire gamut of character-traits and to show in each case that the trait is a product of society. But there is more concrete proof that human nature is a social phenomenon, primarily, and that the individual is an individual by virtue of his membership in a group. Infants raised in isolation lack personality in proportion to the extent of their isolation. The story is told of two girls found living in a cave in India, with only wolves as companions. One of the girls was evidently about twelve years of age, and the other, eight. How they came to be living there was a mystery and no one in the vicinity seemed to know how long they had been isolated. According to reports,[3] these abandoned children had no language; the only sounds they could utter were guttural-like growls and weird cries. In their habits they were more like animals than human beings. They ate from the ground, as wolves do, and seldom used their hands. They did not walk erect. They were terrified when their discoverers captured them, and bit and scratched in self-defense. They were taken to an orphanage where attempts were made to civilize them. At first they refused to wear clothes. They took no interest in the other children, in fact, in no human beings other than the matron, save for infants in the creeping stage. The latter were a source of interest, for they were creatures which the "wolf-girls"

[3] Squires, Paul C., "'Wolf Children' of India," *Amer. Jour. of Psychol.*, Vol. 38, 1927, 313-314.

could understand. They would romp around with them on hands and knees, but if the infants did something which evidently seemed strange the "wolf-girls" would pounce upon them and bite them.

If this account is true, and there is no reason to believe that it is not reliable in its essential points, the fact under discussion is illustrated in two ways. First, the orphans possessed no exclusively human characteristics other than the few which could be derived from their own society, because they lacked a human environment. Second, in their behavior they were more like animals than human beings, because they had lived in an animal society. In each case the character of the whole conditioned the character of its parts.

The thesis has been presented that human nature is not an endowment from heredity. To be sure, the infant enters the world with a capacity to grow, to absorb nourishment, and to maintain the organization of his energies, but the direction of that growth and the form of organization of these energies are strictly products of group life, so far as their psychological aspects are concerned. There are no innate properties of the nervous system, yet known, that guarantee a personality, other than a complexity and specialization of structure that make possible highly intricate modes of behavior. The hypothetical man, developing in isolation, would exhibit no traits of character, no intelligence, skill, morality or self-consciousness, and no attributes like aggressiveness, modesty, acquisitiveness, and shyness. He would not exhibit these traits because there would be no social conditions present to create them.

It is possible to summarize the relationships of the individual to the group in the form of definite laws. These are known as *organismic* laws because they rest upon the fact that a society is an organism, or in other words, an organized whole. A discussion of these laws will bring out many details intended to illustrate how the individual acquires a personality and why he behaves like a human being.

First organismic law: The whole is more than the sum of its parts; it possesses properties and exhibits behavior which its parts, taken alone, do not exhibit. In the illustration of the army it was evident that the whole is more than the sum of its parts and that organization describes the "something more." But this is not all. With organization come new properties not possessed by the parts. In the army these properties may be described as a fitness to achieve particular goals, oneness of purpose, and a strength that comes with unity. Organization, then, takes on properties in its own right; the whole possesses attributes that are unique.

Before applying this law to the problem of the social group, as such, consider another example of the same principle. ''Consciousness'' is a property of living organisms of a certain level of complexity. By ''consciousness'' is meant a mode of behavior of the organism-as-a-whole toward the forces of a physical and social environment. This property, or mode of behavior, is not exhibited by isolated parts of the organism. A man's brain, taken out and laid on the table, would not think; an eye, dissected out, would not see; a hand, severed from the body, would not be capable of feeling anything. And if the organism were completely out of contact with its environment the property of ''consciousness'' would not, it is believed, be exhibited.

This same principle, in fact, holds in the physical realm. Reduce water to its ingredients, hydrogen and oxygen, and note the difference between the whole and its isolated parts. Water, an organized molecule, assumes the form of a liquid at normal temperatures; it enters into a variety of particular relationships with other chemical compounds; it sustains life; turns water wheels; holds substances in suspension; and dissolves solids. Compare these modes of behavior with those of the two gases, hydrogen and oxygen. How different they are!

Similarly, social groups, as organized wholes, possess properties and exhibit behavior not characteristic of human beings in isolation. *First,* human nature is one of these properties. Strange as it may seem it is primarily a group, not an individual attribute, for if it were the latter, isolated human beings would possess it. Accordingly, individuals take on their human nature by virtue of their membership in the group. Human nature is a pattern, a group phenomenon, and pertains to the individual only as a part of the whole. *Second,* there are specific modes of behavior that characterize this pattern of human nature. Among them are folkways. Folkways are correct ways of doing things; they are group habits carried out, to be sure, by means of individuals within the group, for that is the way in which all wholes operate, namely, through their parts. The important point to be remembered, here, is the fact that the behavior of the individual in conforming to the group habit represents a dynamic relationship of that individual to the whole of which he is a part. Take the matter of dress. It would make no difference to a man living off by himself whether he wore trousers or a skirt, so long as he was comfortable. A man dresses in accordance with the group to which he belongs. Dressing in certain ways is social behavior; it is a unique property of groups, and it is possessed by individuals only as members of the group. Folkways are uniquely group phenomena.

So too, with elaborate forms of group behavior, like the *mores.* Mores are folkways that have long been practiced and have been raised to the plane of doctrines. Religious observances and political faiths are illustrations. A hypothetical man, reared in isolation, would certainly not exhibit any political behavior, and it is almost as certain that he would have no religion. It is a commonplace fact that a person born and raised in a Catholic family generally remains a Catholic; raised in a Methodist home and community, he generally remains a Methodist. Exceptions in each case can always be traced to outside social influences.

Folkways and mores are relatively permanent characteristics of group life, but even temporary groups, formed by the circumstances of the moment and organized only for the time being, take on properties of organized wholes. The crowd is a good example of a temporary group. The fact that individuals in a crowd will execute performances that when alone they would repudiate, proves the point. The excitement, suggestibility, hostility, egotism, primitiveness and desire of flattery exhibited by the crowd are characteristics that pertain to the group as such; they are properties of the whole, over and above the individuals.

Public opinion, morality and race feeling are also unique properties of groups. Again, a hypothetical man in isolation would be neither moral nor immoral in his reactions because there would be no pattern of human nature to condition the morality of his conduct. It would make no difference to any one if he cut down a tree, or even if he committed suicide. It is evident that a thousand hypothetical individuals, scattered over the land far enough apart to prevent any kind of contact and any knowledge of each other's existence, would have no folkways, no mores, no public opinion, no politics, no right and wrong ways of doing things, no doctrines. But let these same individuals live together in an organized group and all of these phenomena forthwith appear. In the first case there is no pattern of human nature, no whole, no unity, and there is an absence of distinctly human modes of response. In the second, the presence of the pattern, with the modes of response just mentioned, shows that the whole is more than the sum of its parts.

But, the claim is heard, nothing has been said about modes of behavior that persons carry out *as individuals.* What about the desire for food, care of the young, love, anger, fear, thought, and many other so-called performances of individual minds? Self-consciousness, at least, must be innate. This problem introduces the second law.

Second organismic law: Parts derive their properties from the whole. Suppose it be assumed that self-consciousness is an endowment of individuals, for if it is attributed to groups, a group mind would be implied. How do individuals acquire this self-consciousness? The answer is that groups are not self-conscious in a strict sense of the term, although it is legitimate to speak of family feeling, race consciousness, nationalism, and other group attitudes. But group life does furnish the conditions for the development of self-consciousness in the individual.

By self-consciousness, of course, is meant the awareness of personal identity and of the self as the agent of one's acts. Presumably, the first experiences of the infant are those of vague and diffuse tensions set up by internal disturbances of some kind—the forerunners of hunger, pain and excitement. These tensions are either resolved or increased by the behavior of other individuals toward him. He receives food, or is denied it, at the hands of his mother. Later, he is permitted or not permitted by various members of the family, to play with certain toys; hence he learns to contrast his wants with those of others. The rôle played by his head, hands, and feet, in the development of his notion of self, depends upon a comparison of his body with the form and appearance of other persons. Even the impressions, from his own body, of touch, pain and muscular movement, which unquestionably contribute to the concept of self, derive much of their significance from descriptions of similar experiences in others. Important as the infant's own nervous system is, for the development of self-consciousness, it would be impotent if it were not functioning in a social setting. This is because the social setting gives meaning to his experiences, and without meaning there is no experience. So it happens that a very complex pattern of social conditions produces self-consciousness in the individual, and these conditions are the presence of other persons and the attitudes they display through their actions and words.

In all this, there is no implication that self-consciousness is planted in the individual by an act of the group; it is not inserted, already made, into the mind of the child. The child must *acquire* self-consciousness, but he acquires it just as an electrical charge acquires its potential by being in dynamic relationship with different degrees of potential elsewhere. The social group, then, produces self-consciousness in the individual just as a system of stresses produces the particular stress characteristic of any given part of the system. The dynamic relations existing between the individual and the group exhibit themselves in the form of behavior. Under another law it will become evident just what these behavior-processes are.

The individual also acquires *intelligence* through his dynamic relationships to the group. Intelligent performances of other people induce intelligence in him; they function as complex patterns of stimuli which make possible the development of insight. There would be no understanding on the part of the individual if there were no events occurring in his environment capable of having meaning for him. It has already been noted how isolated individuals do not develop intelligence. Understanding an event means that it is noted in its relation to other events; the event is perceived in the light of a total situation. If the situation presents relationships which are simple in their character, such as the jungle environment presented to the "wolf-girls," intelligence remains on a simple level. If the situation presents complicated relationships like those found in a human social environment, intelligence in the individual develops on the average to a correspondingly higher level.

If the "wolf-girls" were real human beings in a strictly psychological sense, endowed with a high degree of native intelligence, why did they not rise far above the level of their animal associates, develop a language, and discover more facts than they did concerning their environment? Were not the events occurring around them complicated enough to stimulate intellectual growth? The answer is this: Before an individual becomes intelligent, such complicated events must occur around him as can be analyzed with the use of his senses, *alone,* especially the senses of sight and hearing. In order to be analyzed, the meaning and cause of these events must be relatively obvious. But, in case of events in the physical environment, causes are not often obvious. What, for example, so violently shakes the limbs of the tree overhead? The relationship of these movements to the wind is not an especially tangible one. What is the relationship of temperature and rain to the abundance of food? Again the problem is exceedingly abstract. Before a man can discover the hidden secrets of the world around him he must have available *extra tools*, to aid his senses. These tools are man-made, and exclusively the products of society. They are inventions in the form of mechanical instruments like the microscope and telescope, instruments of measurement, and what is very important, language and mathematics. They are additional tools with which the intelligent man discovers that the jungle environment of the ''wolf-girls'' is far more complicated than it appeared to them, but his increased insight into that environment hinges upon intelligent responses in other people and upon the mechanical products of intelligence. He has a social heritage which the ''wolf-girls'' lacked. Thus, intelligent behavior in others, together with the mechanical products of that behavior, are stimulus-patterns which induce intelligence in the individual and determine what insight he will possess, even into the nature of the biological and physical worlds. All human insight is gained in this way. Indeed, science is a product of social evolution and the concepts of science cannot be understood, completely, until it is recognized that they are primarily social phenomena. It is, then, the intelligent character of the human nature-pattern that conditions intelligence in the individual.

The individual derives his *emotional life* from participation in group behavior. His first experiences of pleasure and displeasure pertain to food-getting and to the care of his bodily needs. These feelings appear at the outset as aspects of a simple social situation between infant and mother. Throughout life, in fact, experiences of a like character derive their major significance from social settings. Isolated individuals do not love; they do not fight; they do not fear. Love and pugnacity are fundamentally social phenomena exhibited by individuals only as members-of-a-group; they are emotions derived from the circumstance of being in dynamic relation to other individuals within a whole. Man and wife are a social organism of which each is a member, and the love is conditioned by the whole, in this case, the partnership. In contrast to this type of social organism there is the hostile group. Enemies are a social organism and the hate that is displayed between members of such a group is induced by group relationships. It is plain that the group is essential for the development of emotion in the individual.

It might be shown that the simpler and more specialized mental activities of man, such as learning, the perception of time, space, color, and sounds, play their particular rôles in the behavior of individuals as a consequence of life in groups. The fact that man possesses eyes is alone no guarantee that he can see; the fact that there are sensitive organs in the skin is not a guarantee that he can feel the contact of objects that touch him. It is quite likely that, before he can see and feel, his reactions must become differentiated out of a vague and diffused response to all outside stimuli at once. As his reactions differentiate they must be organized; they must mean something; they must possess a biological and social significance. It is not impossible that a social environment is necessary before this differentiation will take place, and that a mere mechanical and chance stimulation of the infant's eyes and skin will not lead to experiences of sight and touch. In order to be effective, stimuli must be available in complex patterns capable of being sensed as wholes. In other words, the situation must be sensible to the organism. An hypothetical infant, exposed only to a physical environment, might never develop a consciousness at all, because the situations that give rise to vision and touch would not possess the biological or social significance necessary for the ordering of his sense impressions. One sees only when there is something to observe that possesses meaning. If these suppositions are true, hearing tones, seeing colors, feeling pains, and the like, are also processes conditioned in part by the social group.

Third organismic law: The whole governs the activities of its parts. This is almost obvious after considering Laws One and Two. Persons are restricted in their freedom by the mores of the group and by all kinds of temporary group standards and public opinion. Numerous are the taboos which keep individuals walking the straight and narrow path. But although mores and customs are group conditions of individual conduct, they do not in themselves tell us *how* the group effects a dominance over its members. What are the interaction processes going on between the individual and the group that describe this type of dynamic relationship? They must be processes going on within the individual, but conditioned by the group. Moreover, they must be processes other than those considered, under Law Two, as the effects of group life. They must be on the order of media or tools through which the whole exercises its control. These tools are imitation, suggestion and sympathy. Here a warning is essential. Imitation, suggestion and sympathy do not explain *why* the group controls the individual; they describe *how* it accomplishes this end; they are procedures, techniques. When a person dresses like those around him, he is imitating them; when he accepts an idea held by the group, the process is called suggestion; when he feels emotions in common with the group, the process is known as sympathy. These interaction processes occur, of course, when the group is as small as two persons.[4]

There are many interesting facts that relevantly come under the discussion of this law. To illustrate: If in the estimation of the group the behavior of a certain member is especially important, the group attitude toward that person makes him an unusually good source for imitation and suggestion. So it happens that a great authority in science is readily believed; a social leader is followed in matters of dress. But the power of the leader and the secret of authority lie in the leader's ability to make definite the hopes, desires and fears of the group, and to help it in times of need. Accordingly, the evident influence that one individual may have over the group is not a reversal of the third law, for unless the leader is recognized by the group he has no influence as an individual. A detailed study of social behavior would reveal many different circumstances under which the third law holds.

Fourth organismic law: Parts emerge from wholes through a process of individuation. The social group supplies the general features of the human nature-pattern. Individual differences are conditioned by variations in the social patterns that influence single persons. For example, the attitudes of parents and other members of the family are not exactly the same toward any two of the children; experiences derived from the care of the first lead to different methods in bringing up the second, and so on. Personality thus emerges through a growth process that commences at birth, if not before, and continues throughout the life of the individual. There is a physiological basis for this, for it is well known that the nervous system is very poorly developed at birth, and it is now believed that maturation continues long after the person has reached the maximum of his height and weight.

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[4] Group control over the individual is also effected through co-operation and motivation, but these social processes should be studied either from the standpoint of sociology, as in the case of co-operation, or from the standpoint of individual psychology (emotion), as in the case of motivation.

The kind of treatment which the infant receives at the hands of the family give to him his first traits of character. If the parents are nervous, quick of action, loud spoken, hasty in speech, and erratic, in their handling of the child, he soon begins to reveal their characteristics, first, by developing a general nervousness, and then, by taking on their specific traits. The emergence, however, is slow. The more refined characteristics of human nature on the one hand, and the more objectionable on the other, do not appear until the individual has passed through the adolescent period and has come in contact with larger social groups. Many and striking have been the alterations of personality effected by changes in social environment.

It is in this general way that a new personality crystallizes out of the total pattern of human nature that surrounds it. The emergence is definitely an individuation from the total pattern, because the circumstance responsible for the appearance of the personality is the constant dynamic relation existing between the individual and the group. As the emergence takes place the group-pattern, through its dynamic relation to the individual, imparts human nature to him.

Fifth organismic law: In the process of evolution, wholes evolve as wholes. In the evolution of society the group did not come into existence as an organized whole by the additive method, beginning with a single individual. The group was an evolutionary unit; large numbers of persons evolved together. In fact, there is reason to believe that biological evolution, at least of the higher animals and of man, has depended in large measure upon the existence of societies. Life in organized groups aided in the struggle against natural enemies and unfavorable climatic conditions; it decreased the chances of infant mortality and aided in the selection of useful biological variations. There is no question but what group life was the outstanding factor in stimulating the development of language, and with it, thinking.

Sixth organismic law. It is possible to suggest a general dynamic and organismic law that underlies all the facts that have thus far been discussed, namely, *the law of least action.* Presumably it is a law that holds everywhere in nature, whether the phenomena under consideration be physical, biological, psychological or social. This law, in common with all others, does not furnish an ultimate explanation of anything, for science does not deal with ultimate explanations. It merely serves as a logical expedient by means of which to organize the facts of science into one related system. It is purely interpretive; it is merely a concept, but its usefulness is measured by the extent to which it makes the facts of science intelligible, and by the problems which it raises. The law of least action is based upon the assumption *first,* that all events have to do with transformations of energy. Energy was formerly regarded as a mechanical concept, but it is no longer considered so by many scientific thinkers.

It is based, *second,* upon the assumption that energy always exists in the form of systems or organized wholes, the parts of which can be described as different degrees of potential, stress or strain. For convenience, a particular gravitation system, say the earth and objects existing upon it, can be taken as an illustration. The five organismic laws that have just been applied to social behavior still hold in a general way. The system is more than the additive sum of its parts; it is an organic whole. The system, as such, conditions the properties of its parts. The weight of a stone, for example, is derived from the system of which the stone is a part. The system, too, governs the activities of its parts. The falling of an apple is conditioned by the system and follows laws that pertain to the system, not to the parts considered out of relation to the whole.

The law of least action, as an organismic law, is based *third,* upon the assumption that bodies move always from a higher to a lower stress until the differential is resolved to a condition of equilibrium. It must be remembered that a high stress, or potential, means nothing except with respect to a low stress, or potential, and *vice versa.* A high charge of electricity goes in the direction of a low charge; currents of air flow from high to low pressure areas; water runs down hill, that is, in the direction of the center of the earth, which represents a point of equilibrium of stresses within the system. But this is not the total picture. In terms of the organismic conception, bodies do not *commence* to move until the system as a whole furnishes the *end* of the movement, that is, until it provides the place at which the stress is resolved. Hence movement commences only when a remote end has been established. The remote end of the movement of a falling body is the earth's center; the remote end of an electric current is the position occupied by the lower potential. The dynamics, then, by means of which the whole governs the activities of its parts are describable in terms of differentials of stress.

Now, for the law itself. *The law of least action states that a body moves toward its remote end over the shortest route in time when energy is multiplied by time.*[5] The physicist always thinks of action in terms of amounts of energy multiplied by units of time. The psychologist's interest in this law is merely a very general one. He is interested in employing the law as a working hypothesis that describes the character of organismic events. It is a law of wholes, not of isolated parts, for what shall be the shortest route in time from a high to a low stress depends upon the alignment of stresses within the system.

As a working hypothesis the law of least action brings psychological and physical events together on common ground, for it points to a similarity of principle exhibited in the dynamics of physical and psychological events, for example, between the dynamics of a gravitation system and the dynamics of purposive behavior in human beings. In other words, the same assumptions (mentioned a few paragraphs back) that make the law logically intelligible in physics also make it intelligible in psychology.

To illustrate, if bodies within a system always move toward a remote end, established before movement commences, it can be said that the movement is *directional;* its direction is conditioned by the system as a whole. To state it differently, movements within the system are *integrated,* defining "integrated" as directed toward an end by virtue of a particular balance or alignment of stresses within the system. So, the motion of an apple falling toward the center of the earth is integrated with respect to the stresses of a gravitation system; the stresses condition the path of the apple, giving to it direction and a remote end.

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[5] There are exceptions to this law, most of which are found in the field of light. The exceptions are movements over the longest route in time. For reasons that cannot be discussed here, physicists prefer to state the law as the law of stationary action. In the last analysis, from the physicist's standpoint, the law is purely a mathematical and statistical one. The more refined the physicist makes his analyses the more evident it becomes that, in order to be intelligible in any other than a mathematical sense, physical phenomena must be given an interpretation that does not lend itself to strict experimental proof, but rather to logical proof.

In connection with the law of least action *cf. The Science of Psychology,* 79-85, 125, 129, 243, 260, 500, 511-513.

Compare this situation with a simple movement, executed by a human being, say reaching for a glass of water while seated at the dinner table. Physiologists and psychologists speak of this as a very highly *integrated* or *co-ordinated* movement, and again "integrated" means directed toward an end. In this case the end is the glass in a certain position and the alignment or balance of stresses giving direction to the movement is in the neuromuscular (composed of nerves and muscles) system. Both the motion of the apple and the movement of the arm follow the law of least action. But the arm-movement is purposive; it is executed by a conscious organism. Here, the organism corresponds to the gravitation system in "directing" the activities of its parts. The more the stresses within the neuromuscular system of the organism are differentiated into nicely balanced patterns, the more complicated the co-ordination of the movements.

The law of least action offers many further advantages to the psychologist which cannot be discussed here in detail. Two examples will suffice. First, the law makes intelligible the fact that conscious organisms are able, the first time they try, to execute very complicated acts, provided their muscles and nerves have reached a sufficient degree of maturity. Newly hatched pheasants run with surprising agility; infants follow a moving light with their eyes as soon as they are able to see the light clearly; children often exhibit rapid strides in manual dexterity. These performances are not stereotyped. The pheasant can turn to the right as easily as to the left; it makes no difference to the infant which direction the light is moved; and various letters are written equally well by the child the first time. Compare these facts with the circumstance that an apple will fall toward the center of the earth quite as readily in China as in the United States, and without ever having fallen before! When conscious organisms are able to make well co-ordinated reactions toward a goal of some kind, without practice, the term *insight* is used to describe the organized character of the performance.

Recall, now, that wholes possess attributes in their own right. With organization come new properties. *Insight* describes the attribute that comes with organized response at the level of conscious behavior.

Second, the law makes intelligible the reason why a given mode of behavior *commences* and why it *ends.* A man behaves only when under tension, and behavior is a process of resolving the tension. The resolving, in turn, is effected by reaching the goal. The act is terminated because reaching the goal completes, relatively at least, the establishing of an equilibrium between stresses within the organism. But note that it is *behavior*—doing something with reference to an environmental objective—that brings about the equilibrium. *The resolution of tension within the organism is synonymous with an approach to the goal.* In this way the law suggests a set of dynamic terms with which to explain various kinds of behavior.

Seventh organismic law. This law states that *an energy system resists the disturbing influences of outside forces.* Work is required to alter a system, that is, to change its balance of stresses. One must expend energy in order to change the position of a stone in the gravitation system of which it is a part. Imagine how much energy would be required to lift the earth out of the solar system! Within limits, then, a system of energy "tends" to retain its *status quo.* This fact, applied to behavior, is apparently the basis of individual habits and of group conservatism. The fear that individuals and groups, alike, exhibit toward the new and strange is an expression of human energy-systems that are in the process of resisting disturbing influences from without, and when this resistance occurs in human beings the process is called self-protection.

This law can be stated in another way as the law of "maximum action" or "maximum work." In terms of this law all of the potential energy available for doing work is expended in the course of approaching equilibrium. This law does not conflict but supplements the law of least action. The greatest possible amount of free potential energy is expended in the line of least action. We have a picture of this state of affairs in the human being when with all his strength he co-ordinates his muscles to the end, for example, of dodging an automobile.

Laws Six and Seven can now be employed in a general interpretation of social behavior. Suppose an individual acts queerly, fails to conform to the mores, or becomes hostile, because of a peculiar combination and sequence of conditions not previously encountered in the group. At once the *status quo* of the group is threatened; tension is immediately induced in the group, and this tension is resolved toward the goal of making the miscreant obey, perhaps of eliminating him. Numerous are the ways in which the group achieves such an end—through arousal of public opinion, by trial, punishment, enactment of laws, and social ridicule. The group attitude, in turn, induces tension within the individual. Then *he* imitates or accepts suggestions in the process of resolving *his* tension. In the same general way one group reacts to another because tension, in the one, induced by the other, demands resolution. The maintenance and use of large standing armies is a procedure by means of which group tensions are resolved; tariff laws are enacted to resolve tensions based upon economic issues; immigration laws are enacted to resolve the tension of race prejudice. In any of these illustrations the procedure is carried out quite as well under one set of conditions as another, for it is always a socially organized reaction consummated in the most direct fashion, under the circumstances, toward a particular goal.

Eighth organismic law. The last organismic law to be presented, here, may be called the *law of configuration.*[6] *Energy exists in the form of systems or configurations of stresses that effect one another as wholes.* Applied to behavior, the law may be stated as follows: *Any reaction of the human organism as a whole is a unified response to a total situation of some kind. If the response is directed specifically toward a detail of the total situation it is always made to that detail in its relation to other details.* As a conscious activity, therefore, the response is insightful. The term configuration is intended to describe first, the integrated, co-ordinated character of the response and second, the fact that it is a response to a total situation.

The full meaning of the law of configuration, however, has not been explained. Consider for a moment all that the expression *unified response* implies in the light of the preceding laws. It implies that an act has begun as the result of tension; that the movements involved are integrated toward an end or goal, established before movement commenced; that the differentials in stress within the muscles and nerves are always so aligned or balanced that the movements occur in the line of least action. The law of configuration involves the principle that the complete situation, just described, is a single, functional unit that cannot be broken down without a destruction of the whole. As a configuration, it is indivisible; each aspect of it implies all of the others; no one part can be defined except in terms of the whole. Tension means nothing in the absence of a goal; the goal is non-existent in the absence of tension; an act, begun, has no meaning without reference to the pre-established end. And finally, the configuration does not exist prior to the tension and prior to the goal, nor does the tension cause the goal or the goal the tension. The same conditions determine both the tension and the goal—the high and the low stress—so that nothing happens unless the conditions for the completion of the act are present before the act commences. The entire configuration comes into existence, and goes out of existence, as a whole. (As the *Gestalt* psychologists say, the whole is *structured;* its essence is its "form"; it is structured both in the dimensions of space and of time. These latter points will be discussed more in detail in subsequent Readings.)

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[6] *Cf. The Science of Psychology,* 76 f.

The social group is just such a configuration. It evolves as a whole and always behaves as a whole. It is a pattern, or field of forces conveniently described as a pattern of human nature. It is a field of potentials existing in the form of personalities; the interplay and balancing of these personalities are social processes that follow the law of least action.

In order to explain group behavior, one must have recourse to laws of group activity. These are organismic laws. The individual must be explained in terms of the group; the parts in terms of the whole; the lower in terms of the higher.

It should not be concluded that the principles here set forth deny the importance and rights of individuals, or disclaim the real existence of personalities. *Personalities are real and they belong to individuals as members of a group.* The main problem under consideration has been: From what sources do men derive their individualities? Neither should it be concluded that organismic principles are mechanistic and materialistic because they apply to physical as well as to psychological facts. They are not. It is now recognized by many scientists (although it has long been recognized by philosophers) that natural laws are not mechanistic and materialistic. The facts of physics can be given a non-mechanical interpretation without altering a single mathematical formula, and without decreasing the chances of exact predictions. The laws of configuration and least action are not mechanistic laws. They are principles pertaining to organized wholes, and organized wholes obey their own laws, no others.

A further study of psychology would reveal the configurational character of modes of behavior other than the social. It is fast becoming evident that all behavior is configurational: Learning, memory, emotion, thinking, perception and sensory processes included; and recently it has been discovered that a similar set of laws hold for the physiological activities of the nervous system. But the examination of these facts is the purpose of the study that has just commenced.

Not only is it apparent that behavior in general is configurational in character but that all facts pertaining to it can be grouped, for the purpose of convenience, under the eight laws here presented. The first law, that wholes possess their own unique properties, may be called the *law of field properties.* Any whole or organized system of energy may be likened to a field. There are fields of gravitation, electrical fields, temperature and pressure fields, fields of potential in the nervous system and fields of consciousness. The human nature-pattern mentioned in this Reading is a field of personalities. Social groups, as fields, exhibit their own unique modes of behavior in the form of customs and attitudes. These are field properties.

The second organismic law, that parts derive their properties from the whole, may be called the *law of derived properties.* It means specifically that parts do not have independent existence and cannot be defined in terms of themselves. The third law, that the whole conditions the activities of its parts, may be called the *law of determined action,* for it is only when we go from the whole to the parts that we find the concept of cause and effect strictly applicable. The fourth law, that parts emerge from wholes, may be called the *law of individuation.* This law portrays the manner in which parts have their origin. The fifth law, that wholes evolve as wholes, may be called the *law of field genesis.* By the expression ''field genesis'' is meant that any whole is a field of energy which evolves as an organized system by expansion and differentiation. The sixth, seventh, and eighth laws, the *law of least action,* the *law of maximum work,* and the *law of configuration,* portray the type of process that goes on in the behavior of wholes with respect to other wholes and with respect to their parts. After one becomes familiar with these laws it should be apparent that they are all aspects of the same general problem, namely, the behavior of energy systems.

READING TWO[1]

THE INFLUENCE OF THE GROUP UPON ASSOCIATION AND THOUGHT[2]

Editor's Note

That dynamic relationships between the individual and the group can be measured under relatively controlled conditions has been demonstrated in recent years by several studies, including the following by Professor Allport. This investigation shows that the group is a factor of influence even in the performance of simple tasks. Group conditions not only alter the speed but also the quality of work done by individuals. If the quantitative results are not striking it should be remembered that the tasks for the most part were relatively simple and insignificant. Nevertheless, the principle is demonstrated that the whole is an important condition for the activities of its parts. Where an individual finds that he can work better in isolation, as in studying, writing a novel, or what not, he is still behaving with respect to a social group. It is the immediate presence of other people to which he objects.

INTRODUCTION

If social psychology is to achieve the title of an independent science, it is high time that its many speculative theories and crude generalizations be subjected to experimental methods. The data of this science, it appears to the writer, may be for convenience subsumed under two heads, *viz.:* (1) the behavior of an individual in direct response to social stimulus, that is, in response to some form of behavior in others, and, (2) behavior which is the response to a non-social stimulus, *e.g.*, a column of figures to be added, or a meal to be eaten, when such response is modified by the presence and actions of other persons. Responses to direct and incidental social stimuli are, in brief, the two classes of data for social psychology.

The following experiments bear upon certain problems of the second class of data mentioned. The method employed was to compare the mental processes (in this case association and thought) of the individual when alone with his reactions to similar and equivalent stimuli when a member of a "co-working or co-feeling" group. In this manner the part played by incidental or contributory social stimulation was determined.

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[1] Reading Two parallels Chapters II and III, Wheeler, *The Science of Psychology.*

[2] By Floyd H. Allport, Syracuse University. From the *Journal of Experimental Psychology*, 1920, Vol. 3, 159-182. By permission. Certain parts of the original article have been deleted where data of correlations were presented; certain of the tables have been shortened.

General Method.—It was considered advisable to eliminate all incentives to rivalry which were not inherent in the very nature of the situation (*i.e.,* individuals working on similar tasks in one another's presence). The subjects were instructed not to regard their work as competitive; overt comparisons between individuals were also prohibited. The time given for the tests was constant, hence no one subject finished before the others. In this way rivalry, which is a distinct social problem and which should be studied separately, was reduced to its natural minimum. Each subject, however, was instructed to acquire the attitude of doing his best in both the group and the solitary work.

The subjects were arranged in groups, containing from 3 to 5 subjects each. The groups had no changes of personnel during a whole experiment. The subjects were upper classmen and graduate students in psychology at Harvard and Radcliffe Colleges. They were 26 in number, though not more than 15 were used in any single experiment. There were 24 men and 2 women. In age they ranged from 20 to 40 years, 26 being the average age.

In the group work the subjects were seated one on each side of a table 3 feet by 5 feet in dimensions. In groups of 5 two subjects sat at one of the longer sides. The same seats were retained by subjects throughout the course of an experiment. Care was taken to secure conditions, such as type of table, light, air, seating of the subjects, etc., in the rooms used for solitary work comparable to those conditions in the room where the subjects worked as a group.

The free chain associations which were to be written were started by a stimulus word, for example "building" or "laboratory," written at the top of a sheet of paper given to each subject. The same stimulus words were employed in the two conditions, *T* and *A*.[3] It was also emphasized in group work that the same stimulus word was given to all. It is not believed that the presence of the experimenter in the group work materially affected the results of the social influence.

In all experiments except the first constant intervals of time were given, in the group by spoken signal, and alone by buzzers

[3] *I.e.,* "Together" and "Alone."

placed in each room and tuned down to inobtrusive intensity. Control tests were given in the group, using the buzzer for signals in order to determine whether the buzzer itself played a part in the results. No difference was found in the average, between group tests given by the buzzer and those given by the verbal signal. The writing materials (pen, pencil, etc.) used by each subject were kept as constant as possible throughout the experiment.

EXPERIMENT I

Free Chain Association

1. *Procedure.*—The first experiment, introductory in character, was the only one in which the amount of work was constant, and the time required to finish was taken as the objective result for each subject. Sheets of paper were ruled for writing 100 words. Three tests were given within the hour. Only one group, consisting of but 3 subjects, was used for the experiment. The experimental hour came once per week, and the experiment lasted about 12 weeks. There were weekly alternations of the social conditions, together (T) and alone (A).

The papers were placed before the subjects face down. At the signal they were reversed and the subjects glanced at the word given at the top and proceeded to write their free associations one below the other. The writing of the successive words of sentences or phrases was prohibited as was also the serial association of numbers. While working together the time for each subject was taken by the experimenter; when working alone each subject timed himself with a watch or stopwatch. A rest of three minutes was given between test sheets. Immediately after the completion of each test, both together and alone, the subject was required to mark with distinguishing symbols certain kinds of ideas, as follows: (1) *Personal associations* (egocentric), that is, words related directly and intimately with the subject's own past as experience not likely to play the same part in another's association trends; and (2) words which were written without discoverable connection with any of the preceding words, that is, "free rising ideas."

2. *Treatment of Data.*—In the following pages the term "social increment" is used to indicate a gain in the average quantity of work done in the group over the average done alone. "Social decrement" indicates a loss in quantity in the group per-

formance. Corresponding gains and losses in *quality* of the work in the group are termed "social supervaluents" and "social subvaluents" respectively. (The social increments, decrements, super- and subvaluents, are always expressed as a percentage of the average quantity or quality of the work done alone.) In this experiment the individual social increments or decrements are given on the right in Table I.

The results expressing the social influence upon the kind of associations (personal, objective, etc.) were too meager to admit conclusions. They are therefore omitted for this first experiment.

TABLE I

AVERAGE TIME SCORE FOR 100 WORDS
(No. of trials for each individual: Alone 9, Together 12)

Subject	*Alone*	*Together*		*Per Cent.*
Bar	4.3	3.9	Gain *together*	9.3
Stu	5.8	5.0	Gain *together*	13.8
Lan	3.3	3.4	Gain *alone*	3.0
Average	4.4	4.1		

3. *Discussion of Results.*—Two out of three subjects have a social increment. Both of these increments were much greater than the social decrement of the other subject. For the group there was an average gain in time over solitary work of three tenths of a minute. The first rough indications therefore point toward an increased number of free associations produced in the group.

EXPERIMENT II

Free Chain Association

1. *Procedure.*—The study of free associations under the social influence was now continued using a larger number of subjects and tests. The groups used all came twice within a week, the experiment lasting three weeks. There were 15 subjects arranged in 3 groups of 5 subjects each. It was decided to use fairly frequent alternations of the conditions T and A, thus equalizing effects of practice. One group (C) underwent alternations of "together" and "alone" *on the same day:* sometimes two and sometimes four alternations. A combination was used of the sequences $T\ A\ T\ A$ (or $A\ T\ A\ T$) and $A\ A\ T\ T$. If the tests of a certain day began with T and ended with A,

those of the following day would begin with *A* and end with *T*. The entire series moreover began with *T* and ended with *A*. Hence the initial lowering due to newness of the task and the final possible increase due to practice would favorably affect only the score for work done *alone*. The other two groups (*A* and *B*) changed their social conditions on successive days. Group *A* began with a day in *T* and ended the series with *A*; group *B*, as a check, began the series with *A* and ended with *T*.

The routine of this experiment had one important difference from that of the preceding. The *time* was now the constant factor, and the *number of words written* was the measure of the association process. The interval of each test was three minutes, given with a suitable preparatory interval, by voice in the groups, and by buzzer when the subjects worked alone in separate rooms. A short rest period was allowed between tests.

Another important variation was the division of the work of each test into three *periods* of *one minute each*. After one minute of the time had elapsed the experimenter (in the groups) directed "draw line," whereupon each subject quickly made a line under the word he was then writing or had just written, and then continued with his work. This was repeated at the expiration of the second minute. When the subjects worked alone these signals for the divisions were given by short strokes of the buzzer. The rooms of the subjects when working alone were interchanged from day to day in order to obviate the effect of the peculiarities of any one room upon the work of a subject.

To the types of associations required to be marked in Experiment I were added two more, *viz.:* (1) words, other than the first, suggested mainly by the stimulus word; and (2) words suggested by the immediate surroundings. Introspection was required immediately after each test together with a rough estimation of the degree of the rivalry consciousness expressed on a scale of 0 to IV. In all other respects not mentioned the procedure was the same as that in Experiment I.

2. *Treatment of Data.*—The individual tables are omitted because of lack of space. Table II presents the group average. Practice effects at the beginning which appeared in the individual records were eliminated from the averages by the following rule. In the first day's work (group *A* and *B*) all tests which are lower than every single score made in tests on later days are ruled out.

TABLE II

AVERAGES OF ASSOCIATIONS

15 Subjects	*No. Trials*[4]		*Alone*				*Together*			
			No. Assoc. per Min.			*Total No. Assoc.*	*No. Assoc. per Min.*			*Total No. Assoc.*
	A	*T*	*1st Min.*	*2d Min.*	*3d Min.*		*1st Min.*	*2d Min.*	*3d Min.*	
Average	11.4	13.5	20.8	19.5	19.8	60.3	22.2	20.8	20.5	63.6

3. *Discussion of Results.* (*a*) *Quantity of Associations.*—Table III presents the average number of associations, together and alone, for each subject, together with the per cent. of gain under the social condition to which it belongs. We find our first experiment amply verified. 93 per cent. of the subjects (14 out of 15) produce more associations in the group than they produce alone. The social increments are not large, but their preponderance is conclusive. Cut, Rob, and Sto have considerable social increments (from 12 per cent. to 16 per cent.). The average social increment also of the 14 is twice as great as the social decrement in the case of the one exception to the favorable group influence, Hun. The number of associations produced by all subjects together is also slightly greater than their average alone (63.6 to 60.3).

The mean variation among the subjects is higher, relatively to its mean, in group work than in solitary (8.5 compared with 7.6). Hence we find increased variability accompanying a social influence toward increased mental activity.

Let us now consider the *distribution of the social increment.* The question proposed is whether the increase due to the presence of the group was equally distributed, or whether it occurred chiefly at the beginning, the middle, or the end of the three-minute period. We may compare the results of the three-minute periods both in number of subjects who have social increments in those minutes respectively, and also in the percentile value of the gain in group-work shown in the average of all subjects in the three minutes respectively. This latter comparison is taken from the averages of Table II. We may speak of this gain as the "group social increment." Table IV presents the above relations.

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[4] Exclusive of the trials eliminated owing to effect of practice.

TABLE III

PERCENTILE GAINS IN AVERAGE NUMBER OF ASSOCIATIONS

Subject	*Av. No. Associations*		*Per Cent. of Gain*	
	Alone	*Together*	*Alone*	*Together*
And	56.	58.3	..	4.1
App	62.7	63.4	..	1.1
Cut	67.8	78.7	..	16.
Hor	54.3	54.4	..	.2
Hos	44.8	48.3	..	7.8
Hun	56.8	55.1	3.	...
Kno	57.3	58.2	..	1.5
Lan	56.8	61.2	..	7.8
Pep	68.4	71.2	..	4.
Pre	69.9	72.4	..	3.7
Rob	65.	73.	..	12.3
Spe	67.4	67.5	..	.1
Sto	40.1	46.7	..	16.4
Tul	76.9	80.9	..	5.2
Woo	61.4	65.7	..	7.
Average	60.3	63.6	3.	6.2
Mean variation	7.6	8.5	..	4.1

Number of subjects having higher average number of associations *together*	14
Number of subjects having higher average number of associations *alone*	1
Number of subjects having equal average number of associations *together* and *alone*	0

In both proportion of subjects having a social increment and in the amount of the increment itself, we thus find that the superiority of the group condition in speed of associations exists throughout the test, but is greatest in the first minute and least in the third minute. The second minute is not far below the level of the first, the drop in increase due to the social influence coming well toward the end of the task.

TABLE IV

		1st Min.	*2d Min.*	*3d Min.*
No. of subjects having greater number of associations *together*		13	12	9
No. of subjects having greater number of associations *alone*		1	3	6
No. of subjects having equal number of associations *T* and *A*		1	0	0
Group Social Increment	Amount of time excess of *T* over *A*	1.4	1.3	.7
	Per cent. of excess of *T* over *A*	6.7	6.6	3.5

If we compare the three one-minute periods with each other in the two social conditions separately (Table II) we find the following. The averages of all subjects alone (20.8, 19.5, and 19.8) indicate a drop in the second minute, followed by a slight rise (probably an end spurt) in the last minute. The averages together, on the other hand (22.2, 20.8, and 20.5) form a steady decrease to the end. This result suggests that the effect of the group is at first a stimulating and later a steadying one.

Another possible interpretation seems to be that during the first minute when the associations come with great facility, the social influence counts for a relatively greater addition of speed than toward the end of the test when, through fatigue and comparative exhaustion of complexes and vocabulary, the facility of writing associations has decreased. Under difficult conditions therefore being alone tends to favor concentration. Group work on the other hand contributes no such benefit to the final and more difficult stages of the task.

(*b*) *Quality of Associations.*—The four general types of association, mentioned under procedure, were counted in each individual test. The average number of each type per test for each subject, in group and solitary work separately, is given in Table V.

Table V

Average Number Associations of Various Types

15 Subjects	*Alone*				*Together*			
	Personal	*Free Rising*	*Sugg. by Stim. Word*	*Sugg. by Surroundings*	*Personal*	*Free Rising*	*Sugg. by Stim. Word*	*Sugg. by Surroundings*
Average	11.4	1.1	.6	1.6	16.5	1.2	.5	2.9
M. V.	8.1	.8	.6	1.3	10.3	.9	.4	2.7

	No. Subjects Having Greater No. Alone	*No. Subjects Having Greater No. Together*	*No. Having Equal No. Alone and Together*
Personal Assoc.	12	3	0
Free Rising Assoc.	5	9	1
Assoc. Sugg. by Stim. Word	7	4	4
Assoc. Sugg. by Surroundings	5	7	3

The first type, and the one yielding the clearest result, is that of *personal* associations. 80 per cent. (12 out of 15) of all the subjects wrote down more personal associations *alone* than together. There is evidently some sort of attitude assumed by the individual in the group which takes him "out of himself" and directs his ideas toward outside objects, and as we shall show later, to the actual presence of the others. In the group we are inclined to expand in our thought; we become objective rather than egocentric, present rather than retrospective.

Secondly, *words suggested by the immediate surroundings* appear to be more numerous in the group than in the solitary condition. If we consider the average of all subjects, the tendency is marked, for the average together is almost twice as great as that alone (2.9 to 1.6). Individuals considered, we find that 3 show no tendency either way while 7 produce more words relating to the surroundings in the group, and 5 produce more alone. Hence we find that an environment of active persons is more likely to intrude upon one's trend of thought than an environment of mere space and furniture such as in the solitary condition.

The third type of associations, the "free rising" ideas, also occur more frequently in the group. The averages give only a slight increase for the group work; but the individual records show that 64 per cent. (9 out of 14) of persons affected either way produce more of these spontaneous ideas in the group than they do alone. It is possible that "free rising" words result here from that greatly facilitated flow of associations characteristic of work in the group.

Finally, we may note that *words,* other than the first, *suggested mainly by the stimulus word* are more numerous in the solitary than in the group condition, as shown by the average of the individuals and especially by the number of individual cases (ratio of 7 to 4). The explanation, though obscure, may lie in the longer persistence of the original trend of thought in the solitary than in the social setting.

(*c*) *Introspection.*—There was substantial evidence from the introspection of awareness of being "drawn out" by the presence of the group, so as to produce associations of a more objective type, as previously shown in the results. As to the group influence on speed, two clear cut factors appear in some cases in the same report. The first is an *impeding* influence owing to sensory distraction, emotional factors such as over-stimulation in rivalry, self-prejudicial comparisons with others, and the like. The second and stronger factor is *facilitation.* Numerous stimuli indicative of the rapid work of one's neighbors serve as a drive to greater effort. The principle here involved is probably that commonly known as "suggestion" or "imitation" in superficial accounts of group and crowd phenomena.

Experiment III

Free Chain Association

1. *Procedure.*—The method used in Experiment II was considered imperfect on the following ground. Associations are as a rule produced more rapidly than they can be written, and therefore the writing down of each one in turn does not allow the most rapid play of which the process is capable. The results may measure, not the associational ability itself, but simply the speed of writing. A short experiment was accordingly prepared in which the subject was to write down, not *every* word, but every *fourth* word which occurred to him. With a little practice the subjects rendered automatic the rhythm of writing every *fourth* word only. The division of the three-minute period into 3 parts was not made in this experiment. The subjects and grouping were the same as for Experiment II. The tests were presented on two days, within the period of a week. The sequences of alternation were as follows, the horizontal line dividing the work of the two days:

Groups A and C		*Group B*	
T			*A*
T			*A*
	A	*T*	
	A	*T*	
T			*A*
	A	*T*	
———	———	———	———
	A	*T*	
	A	*T*	
T			*A*
T			*A*
	A	*T*	
T			*A*

2. *Treatment of Data.*—The number of associations written in each test was multiplied by 4 and tabulated. The record of Tul is omitted owing to insufficiency of data. The result of the first test was eliminated whenever it was found to be lower than the score of any succeeding test under the same social condition. Table VI presents the usual summary of individual records.

3. *Discussion of Results.*—The results shown in Table VI indicate again a distinct though less pronounced advantage for work done in the group. Two subjects show a social equivalence. Of the rest 66 per cent. produce more associations in the group. The group average for the associations together is greater than the average alone, and the average of the social *increments* is very much greater than the average of the social decrements.

TABLE VI

COMPARISON OF AVERAGE NUMBER OF ASSOCIATIONS. (ALSO PERCENTILE GAINS)

Subject	*No. of Trials*		*No. Av. Associations*		*Per Cent. of Gain*	
	A	*T*	*Alone*	*Together*	*Alone*	*Together*
And	5	6	130	130	...	...
App	6	5	150	158	...	5.3
Cut	6	5	166	163	1.8	...
Hor	6	6	113	117	...	3.5
Hos	6	6	80	85	...	6.2
Hun	5	6	82	82	...	...
Kno	6	6	89	101	...	13.4
Lan	6	6	117	125	...	6.8
Pep	6	6	113	111	1.7	...
Pre	6	6	119	126	...	5.9
Rob	4	6	121	118	2.4	...
Spe	5	6	126	129	...	2.4
Sto	6	6	54	60	...	11.1
Woo	4	5	120	119	.9	...
Average	5.5	5.7	112.8	116	1.7	6.8
M. V.	...	...	21	20.1	.4	2.7

No. of subjects having greater number of associations *together* 8
No. of subjects having greater number of associations *alone* 4
No. of subjects having equal number *together* and *alone* 2

The number of trials was too small to allow the mean variation to carry much significance. Comparing, however, the mean variation of the social increments with that of the decrements, we find distinctly more variability in the former. In other words the social influence, when it affects workers favorably, affects them also in very varying degrees. Considering, not increments, but actual number of associations written together and alone, the variability both relatively and absolutely is greater alone.

EXPERIMENT IV

Free Chain Association

1. *Procedure.*—Since the conclusions of Experiment III were based on rather few results the experiment was repeated using

fewer subjects but about twice as many tests. This time no stimulus word was given: the subject thought of his own initial word. Another difference was that every *third* word, instead of every *fourth*, was written. The subjects numbered eight and were divided into two groups of 4 each. The experiment covered about five weeks, each group being tested once per week. There was an average of four tests in each hour that the groups were tested. The sequence of social conditions employed a combination of alternations in successive days and alternations within the same day. Group *A* began with *T*, and group *B* with *A*. The individual summaries are presented in Table VII.

TABLE VII

COMPARISON OF AVERAGE NUMBER OF ASSOCIATIONS. (ALSO PERCENTILE GAINS)

Subject	*No. Trials*		*Av. No. Associations*		*Per Cent. of Gain*	
	A	*T*	*Alone*	*Together*	*Alone*	*Together*
Aza	9	9	25.5	27.5	...	7.8
Cur	8	10	18.6	19.6	...	5.3
Han	5	4	35	37	...	5.7
Ric	9	15	24.9	22.8	8.4	...
Rot	8	16	16.9	17.3	...	2.3
Sli	9	10	19	20	...	5
Tan	8	16	29	29.6	...	2
Tay	7	9	47.1	42.8	9.1	...
Average	8	11	27	27	8.7	4.7
M. V.	..	..	8.2	7.1	.35	1.7

Number of subjects having greater number of associations *together*.... 6
Number of subjects having greater number of associations *alone* 2
Number of subjects having equal number of associations *together* and *alone* 0

2. *Discussion of Results.*—The general results of this experiment verify those of the one preceding. A somewhat greater proportion of subjects (75 per cent.) have a social increment. This proportion is still, however, less than the 93 per cent. who attained social increments in the mechanical task of writing down *every* associated word.

The average increment is less than the average decrement owing to the unusually large decrements of the two subjects who did better alone. This fact probably justifies the conclusion of individual differences, the two subjects, Ric and Tay, requiring the solitary condition for their best efforts. Work upon reasoning (see Exp. VI) corroborates this tendency in the case of Ric.

EXPERIMENT V

Controlled Association—Contrasted Occupations

As a minor digression from the usual setting of the experiments, an attempt was made to compare not the work of the subject alone with his work in the group, but to compare his accomplishment, always in the group, in tasks *similar* with that in tasks *opposite* to the occupations of his fellows. Half of the group were seated at one side of the table and instructed to write words all bearing upon one specified topic (*e.g.*, winter), while the other half, seated opposite, wrote words all bearing on the opposite theme (summer). The solitary condition was not used in this experiment.

A test pair consisted of two performances upon a given theme, one the result of working in the manner described above, the other produced at another time when all members of the group wrote upon the *same* topic. Only about three such test pairs were obtained from each subject. Their total for the experiment was 35. Of these pairs 19 showed an excess of associations written in the common occupation over those written in contrasted work. In only 16 pairs was the advantage with the contrasted occupation. The average excess also in the common work was greater (7) than the average excess where it occurred in the contrasted setting (5).

So far as these results go there seems to be an advantage in tasks in which all are working in agreement over work done while pursuing diverse trends of thought. The tests given are, however, too meager for certainty; they indicate merely an interesting possibility for further investigation.

EXPERIMENT VI

Thought Process

1. *Procedure.*—An experiment was finally performed which extended the study of the social influence to the more intellectual functions involved in reasoning. Statements have been made by various writers that this ''higher'' quality of process is better performed in solitude. Tests of critical and original thinking were therefore devised in the nature of discursive reasoning.

Short passages were selected from the works of Epictetus and Marcus Aurelius which admitted of considerable argument, for and against. The task of the subjects was to write down all the arguments, as many and as strong as possible, which they could think of to *disprove* the point made in the passage given. The epigrams both together and alone were presented in legible handwritten form, one copy to each subject. At the beginning of the group tests it was emphasized that they were all writing on the same statement. The time allowed for writing the ideas in a single test was 5 minutes. A separate passage was used for each test.

Nine subjects were used, arranged in two groups, *A* and *B*. Approximately 20 tests were given alone and 20 in the group. The total period covered by the experiment was 2 months. The social condition was changed (from *A* to *T*, or from *T* to *A*) on successive days. Group *A* began with *T*, group *B* with *A*. The passages selected naturally varied somewhat in suggestiveness; but it is believed that in a series of 20 a fair uniformity was obtained for the two social conditions. This is still more likely since only two authors were used, and those two are singularly constant in the tenor of their utterances.

2. *Treatment of Data.*—Each test written was graded as to quality of the arguments proving the negative of the statement. For this purpose the following scale was used. A distinct, clear, and (for the subject) forceful idea going directly to the question received a score of 3. Developments, extensive illustrations of the point made, giving pertinent opinion of an authority, refining or re-stating the question to remove its logical objections, suggesting an alternative proposition, and the like, each counted 2. Emphatic statements or interjections of rejection, quotations merely stating the opposite, personal aphorisms, repetition of an argument already given, qualification or withdrawal of a former argument, rather irrelevant arguments, and so on, were each scored 1. For each subject each type of idea or argument thus contributed its proper score to each test; and by averaging the sums of these scores for the various tests there were found the average individual scores for ideas. These results are presented in Table VIII.

In addition, the total number of ideas of the three types, together and alone, found in the work of each subject, were averaged separately. Table IX presents this comparison of

TABLE VIII

REASONING

Average Scores for Ideas

Subject	*No. Trials*		*Score of Ideas*		*Per Cent. of Gain*	
	A	*T*	*Alone*	*Together*	*Alone*	*Together*
Aza	19	22	5.6	6.6	...	18
Cur	19	16	7.5	7.6	...	1.3
Han	15	24	12.6	14	...	11
Ric	23	19	9.7	9.7	...	...
Rot	17	20	4.5	4.8	...	6.6
Sli	19	20	9	10.2	...	13.3
Tan	23	19	7.7	8.2	...	6.5
Tay	2	5	9	10.2	...	13.3
Wil	12	15	7	8.4	...	20
Average	16.6	17.7	8	8.8	...	11.2
M. V.	...	...	1.7	1.9	...	4.9

Number of subjects having higher idea score *together* 8
Number of subjects having higher idea score *alone* 0
Number of subjects having equal idea scores *together* and *alone* 1

TABLE IX

AVERAGE NUMBERS OF IDEA TYPES

9 Subjects	*Average Numbers per Test of the Various Types; 3's, 2's and 1's* [5]					
	Alone			*Together*		
	3's	*2's*	*1's*	*3's*	*2's*	*1's*
Average	1.97	.8	.31	2.1	1.0	.36

averages of idea types. Taking the number of each type as a per cent. of the total number of ideas in the given social condition (*i.e.,* together or alone), one may compare the relative contributions, together and alone, of the several types to the total idea score for the subject. This comparison is shown in Table X. Finally the words written in each test were counted, and the average number of words per test for the different individuals, together and alone, was computed. Table XI contains the group average together with the percentile gains under the proper social condition.

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[5] These figures are *numbers* of cases only. The idea scores, as given in Table VIII, were obtained by multiplying the number of 3's by 3, the 2's by 2, and 1's by 1, and then adding these products. This was done in the separate tests, however, since the averages given in Table IX contain small inaccuracies due to decimals.

TABLE X

PERCENTILE COMPARISON OF IDEA TYPES

9 Subjects	*Per Cent. of Average Total Ideas Comprised by Ideas of the Various Types—3's, 2's, 1's*					
	Alone			*Together*		
	3's	*2's*	*1's*	*3's*	*2's*	*1's*
Average	63	26	11	60	29	11

COMPARISON OF PERCENTAGES

Type 3—	Number of subjects having higher per cent. for type 3 *together*	3
	Number of subjects having higher per cent. for type 3 *alone*	6
Type 2—	Number of subjects having higher per cent. for type 2 *together*	4
	Number of subjects having higher per cent. for type 2 *alone* (1 subj. equal)	4
Type 1—	Number of subjects having higher per cent. for type 1 *together*	6
	Number of subjects having higher per cent. for type 1 *alone*	3

TABLE XI

AVERAGE SCORES FOR WORDS WRITTEN (GROUP)

9 Subjects	*Score of Words*		*Per Cent. of Gain*	
	Alone	*Together*	*Alone*	*Together*
Average	96	102	2.6	16.6
M. V.	32.1	30.3	1.1	10.2

No. subjects having higher word scores *together*	6
No. subjects having higher word scores *alone*	3
No. subjects having equal word scores *together* and *alone*	0

3. *Discussion of Results.*—From Table VIII it will be seen that of the 9 subjects used, 1 had equal average idea scores together and alone. The remaining 8 *all* had higher idea scores when working in the group. The average idea score together for all subjects showed also a social increment: it was 8.8, while the average alone was 8. The individual social increment, generally rather large, ranged from 1.3 per cent. to 20 per cent., 5 out of 9 being above 10 per cent.

Turning to the percentile importance of each idea type alone as compared with its importance together, 6 out of 9 subjects had a higher percentage of superior ideas (counting 3) while working alone. The number of the subjects, together and alone, having the higher percentage of "2" ideas was *equal.* There were (reciprocally to the "3" class) just 6 out of 9 subjects who had a higher percentage of the lowest type of ideas (counting 1) while working in the group. The averages of all subjects indicate also a higher performance, relatively, so far at least as the proportion of superior ideas is concerned, while working alone. There is thus demonstrated a social subvaluent for argumentative or discursive reasoning. This finding is no doubt in accord with commonly observed facts of life. Who has not been aware, upon retrospection, of the low order of logical value in many arguments given under such a strong social influence as that of political meetings and oral debates? There seems to be a *spreading out* of our thought rather than a strong output of separate original ideas of logical worth. Group thought is *extensive;* individual thought is, to some extent, *intensive.*

May not this "extension" in group thought be also characterized as "wordiness"? It seems quite logical to call it this, for 6 out of 9 subjects wrote more words in the group than they did alone. The averages of the individuals' scores also show the group gain in number of words written (102 to 96). A third evidence lies in the excess in the average of the social increments (16.6) over the decrements (2.6). This disclosure is consistent with the results of all previous experimentation on the social influence. There has been throughout a clear increase of the *quantitative* aspect of mental processes and mental work in the group condition. Our association experiment of writing every word in free thought may be compared with the writing of every word in controlled thought in the present experiment. In both cases every subject but one showed, in the quantity of words written, a distinct social increment.

4. *Introspection.*—There is some evidence in the reports of the awareness of the social subvaluent in the thought process.

SUMMARY OF CONCLUSIONS

A. The Influence of the Group upon Association

I. *Quantitative Aspects*

1. The main result of the preceding experiments on association is the conclusion that the *presence of a co-working group is distinctly favorable to the speed of the process of free association.* In various tests from 66 per cent. to 93 per cent. of the subjects show this beneficial influence of the group.

2. The beneficial group influence is *subject to variation according to the nature of the task.* In the more mechanical and motor requirements, such as writing *each word* associated, the group stimulus is more effective than in the more highly mental

or more purely associational tasks such at writing only every *third* or *fourth* word.

3. There are *individual differences* in susceptibility to the influence of the group upon association. One type, who are nervous and excitable, may succumb to the distracting elements of the group activity and may show either no effect, or else a social decrement.

4. *In its temporal distribution* the beneficial effect of the group is greatest in the first part of the task and least toward the end of the task.

5. There is a tendency for the *slow individuals to be more favorably affected* in speed by the group co-activity than the more rapid workers. There are, however, certain striking exceptions.

6. The *variability in output* among the individuals varies generally with the social influence. Hence it is usually greatest in the group work. A striking exception to this occurs in the tests where rivalry is correlated with the social increment, and where only every third or fourth word is written. Here the variability is greatest in the solitary work. This result is in agreement with that of earlier investigators working on different processes.

7. There is suggestive but not conclusive evidence that the output of associations in a group where all the members are forming associations in the same category is greater than that in groups in which the members are divided in the trend of their associations between opposite or contrasted categories.

II. *Qualitative Aspects*

8. A greater number of *personal associations* are *produced alone* than in the group.

9. In harmony with this fact is the tendency for subjects to produce *ideas suggested by their immediate surroundings* with *greater frequency in the group* than alone.

10. Less clear cut, but very probable, are the tendencies to produce a *greater number of "free rising" ideas in the group,* and to produce a greater number of words *suggested mainly by the initial stimulus word when working alone.*

III. *Factors in the Social Influence*

11. There are two opposing groups of factors in the influence of the social condition upon the association process. They are:

(1) Facilitating Factors:

(*a*) *Facilitation of movement* by perceptions or ideas of movements in others near us.

(*b*) *Rivalry* intrinsic in the bare social setting of a group working together. Rivalry is well correlated with the beneficial influence of the group in tests of a more mental sort (and less mechanical) such as writing every *fourth* word only. It is not so correlated when each word is written.

The beneficial effect of the group in experiments where the rivalry consciousness is closely correlated with this influence is less than in experiments where it is not so correlated, but where other factors—for example, motor facilitation—serve as the stimulus of the group.

(2) Impeding Factors: distraction, over-rivalry, emotions. Of the two groups, the facilitating is by far the more important in the total effect upon the work.

12. Besides the comparisons already indicated, we may note the general agreement of our work with that of earlier students in the *speed* improvement of mental operations, as shown by the quantity of the product, under conditions of working with others.

B. THE INFLUENCE OF THE GROUP UPON THE THOUGHT PROCESS

13. In the highly controlled association of the thought process, as typified in written argument, more ideas are produced in the group than when working alone. Again we find an increased flow of thought owing to the social stimulus.

14. Among the ideas so produced, those of superior quality, however, are of relatively greater frequency in the solitary than in the group work. Ideas of a lower logical value are relatively more numerous in the group work.

15. More words are used in the arguments produced in the group than in those produced in solitude.

16. From the above facts, and also from the introspection of the subjects, we may conclude that the presence of the group influences the reasoner toward a more conversational and expansive form of expression. The more intense logical thinking of solitude gives way in the group to extensity of treatment.

17. These results appear to be related to the common observation that work requiring imagination or more concentrated and original thought is best performed in seclusion. There is also a connection suggested with the writer's experiments upon the

social influence in attention and mental work. In that investigation, as well as in the present, the social influence was found to improve the quantity but not the quality of the mental performance.

GLOSSARY

Complexes	groups of related words or ideas recalled more or less as wholes.
Drive	incentive; "urge."
Egocentric	the condition of being occupied, or inclined to be occupied, with situations and ideas relating to the self.
Free Chain Associations	a series of words recalled at random.
Free Rising Ideas	series of ideas or words recalled in the absence of a definite and obvious system in mind.
Introspection	self-observation; the procedure of analyzing one's own mental processes.
Objective	pertaining to objects or situations external to the individual.
Subject	person being experimented upon; observer in an experiment.
Subjective	pertaining to mental processes as such.

READING THREE[1]

DO GROUPS THINK MORE EFFICIENTLY THAN INDIVIDUALS?[2]

Editor's Note

The method adopted by Professor Watson in the following experiment demonstrates organismic principles in an unusually clear and striking fashion. By virtue of being in dynamic relation to each other and to the whole, individuals in an average group are able to achieve, as a group, not as individuals *per se,* more than the sums of accomplishments achieved under conditions of isolation. From the standpoint of the individual, the dynamic relation reveals itself in increased alertness and by better organization of thinking. From the standpoint of the group, the relation is revealed in the motivating character of the social setting. Taken as a unit, the behavior of the group constitutes a pattern that is better organized and more efficient than the patterns of behavior exhibited by the individuals when alone. Thus, it is shown that group activity is more than the additive products of individual activities and that individuals, performing in a group, take on attributes considerably altered from those revealed when the same individuals work by themselves. This does not mean, however, that under certain conditions, individuals will not work more efficiently if temporarily removed from the presence of the group.

It is readily demonstrated that faith in the practice of group thinking is not uniform. In some business corporations committee action is the rule, in others one good man is selected and made responsible. College students are sometimes set to work upon individual term papers, sometimes upon group projects. Research is usually an individual task, occasionally a group product.

Opinions upon the value of the group unit for intellectual tasks are varied. Some persons believe that they create a better product working by themselves. They lay emphasis upon the self-consciousness, confusion, distractions, interruptions, and irrelevant personality conflicts which arise in groups. Other persons prefer to work as one of a group. They lay emphasis upon the way in which the group stimulates the individual to do his best, suggests to him lines of thought he might otherwise have missed or rejected and corrects the errors of individual bias. Some recent developments in educational and political theory have laid great stress upon the need for group activities. Miss Follet,[3] in *The New State,* elaborates an appealing theory of the integration of individual contributions in a group-thinking process. Overstreet [4] writes, "In every coming together of minds that are serious in the effort to understand, there is something more than the minds. There is the Creative Plus, which no one mind by itself could achieve." A series of directions for discussion groups contains the statement, "Trust the group. There is no person in it who is not superior to the rest in at least one respect. The experience of all is richer than the experience of any. The group as a whole can see further and more truly than its best member."

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[1] Reading Three parallels Chapters II and III, Wheeler, *The Science of Psychology.*

[2] By Goodwin B. Watson, Teachers College, Columbia University. From the *Journal of Abnormal and Social Psychology,* Vol. 23, 1928, 328-336. By permission.

Experimental evidence is limited. Thie [5] in connection with high school classes in English, and Barton [6] in the field of algebra have demonstrated that students working together in small groups progress more satisfactorily in several respects than do students working by more traditional methods. Miss Gordon [7] has shown that the average of individual judgments in such matters as esthetics or in the judgment of a lifted weight is equal or superior to the judgment of the best individual within the group. The larger the number of individual judgments averaged, the more closely does the resulting figure approach the true value. This is encouraging evidence on the quality of the group product.

There remain other questions, among them the question of speed production. The purpose of the experiment to be reported here was to test the intellectual efficiency of a group as compared with the efficiency of the same individuals working

[3] Follett, *The New State*, Longmans, 1920.

[4] Overstreet, *Influencing Human Behavior*, People's Institute Publishing Co., 1925.

[5] Thie, "The Efficiency of the Group Method," *English Journal*, 1925, 14: 134.

[6] Barton, "Group Activity versus Individual Effort in Developing Ability to Solve Problems in First Year Algebra," *Jour. of Educ. Admin. and Super.*, November, 1926, 512-518.

[7] Gordon, K., "Group Judgments in the Field of Lifted Weights," *Jour. of Exper. Psychol.*, Vol. 3, 1924, 398-400.

Gordon, K., "Group Judgments in the Field of Esthetic Judgment," *Jour. of Exper. Psychol.*, February, 1923, 36.

by themselves. The task chosen was that of making up words out of the letters contained in a given word. The subjects were 108 graduate students of education. They were divided into twenty groups varying in size from three to ten. After careful instruction and fifteen minutes of preliminary practice to reduce the handicap of persons who had never done this word-building before, each group was given envelopes containing the first set of basic words, out of the letters of which new words were to be made. Four basic words were chosen, each containing nine letters, four of which were vowels, and one an ''s,'' with no letter appearing more than once in a word. The words were ''Educators,'' ''Neurotics,'' ''Secondary'' and ''Universal.'' The words were rotated among the groups, during the four periods of the experiment in such a way that no ease or difficulty of a particular word would contribute to one period more than to any other. As a matter of fact, as is shown in Table I, the differences between words were slight, ''universal'' being somewhat more difficult than the other three.

The first ten-minute period was spent by each person making as many words as he could, by himself. He sat where no other could see his paper, and wrote down words as fast as he could think of them. After a brief intermission, the group came together for a group-thinking process. The group secretary kept the list of words and each person called his contribution aloud. Ideas, just in the process of becoming suitable words, were expressed in the group, and others helped to supply the desired term or modification. This continued for exactly ten minutes. After a longer period of relaxation the group came together again for a second period of group thinking, opening this time the envelope containing the third basic word. The fourth and final period was like the first one, a period in which each individual constructed as many as he could. Thus practice effect probably contributed about equally to success in the group process and to success in the individual process.

The group averaged five or six persons each. The poorest individual working by himself constructed about 18 words in the ten minutes. The average individual constructed 32 words, while the best in each group constructed 49 words. In the periods of co-operative work, the same persons produced about 75 words, on the average. This, although far more than was produced by the best individual working by himself was slightly less than the group product obtained from a net total of all words produced by those individuals when they were working separately.

TABLE I

COMPARISONS OF DIFFICULTY OF WORDS USED
(All averages are accompanied by S. D. of average)

Scores Made	*Words*			
	Educators	*Neurotics*	*Secondary*	*Universal*
Individual: first trial ...	32 ± 2.9	37 ± 6.3	30 ± 1.4	24 ± 1.4
Individual: second trial .	40 ± 1.8	35 ± 2.2	37 ± 2.7	25 ± 1.2
Groups: first trial	74 ± 6.4	69 ± 3.3	71 ± 1.0	70 ± 5.5
Groups: second trial	86 ± 2.5	77 ± 4.1	87 ± 5.1	63 ± 3.1
Average	58	55	56	46

If comparison be made in terms of the average individual production, the poorest person has an efficiency of 55 per cent., the best of 150 per cent., the co-operative group of 231 per cent. and the compiled group, using the sum total of individual products, would average 268 per cent. In every group the group thinking produced more words than did the best individual of the group. In five of the twenty groups the co-operative group work produced more words than did the compilation of a group total list based upon individual activity.

It would seem fair to conclude that in a task of this sort, building words for a ten-minute period, a group of from three to ten persons can be counted upon to do better work than the average or best person in the group. Only rarely is the group likely to do better work in oral discussion than would be attained if it were possible to add together a sum total of individual contributions.

It may be questioned whether many tasks permit the simple addition of one man's product to that of another. If such situations are few, it is fortunate that the co-operative, oral group-thinking process is practically as efficient as the group compilation. Further experiment is needed to determine whether the group would prove more or less efficient if the task were to involve a long project with many complex adjustments.

Groups are, as one would expect, more alike than individuals, but co-operative group products vary even less than average ability within the groups. A democracy of group units would seem to be not only above the average ability of the individuals concerned, but also much less diverse. Another interesting fact is the wide variation in ability among the persons making low score for their group. This scattering would seem to be a result of selecting groups high on the ability scale so that the poor ones scatter down over a long range.

Size of group seems to be related about equally to net total of individual production and to group production. Larger size apparently increases the superiority of the group product over the average individual work. The average intelligence of the small group, as measured by the Otis Higher Examination, and within the range of the graduate students included in the experiment, is not highly related to any factor. The scores of poorest and best individuals should, of course, show a relation to average individual production within which they play a considerable part. It is interesting to note that the variation in *best* individual production was more potent not only in influencing averages but also in influencing the net total of individual productions. The variations among ''poorests'' were negligible in their influence upon the total product, while the variations among ''bests'' mattered greatly. In measuring the output of a group, either when working along co-operative group-thinking lines, or when the project permits simple compilation of individual efforts, it matters little about the ability of the poorest or even the average person of the group. The result seems to show primarily what the few ablest in the group have produced.

What a group produces depends not so much on average in dividual ability at the specific task in hand, as on average individual intelligence. In other words, group production may be largely a result of factors other than the sort of individual training which education has so often sought to give. These other factors which are important in determining how well a group can do are related to intelligence, although, as suggested, presumably dependent in part on practice in group work.

In studying the influence of the factor of size, when intelligence and individual capacity for production have been held constant, it appears that within the limits of size of groups studied (3 to 10 persons) the larger groups produce more words although as the groups increase in size there is some tendency for the method of compilation from individual reports to increase in its superiority to the method of co-operative discussion.

The experiment suggests the following hypotheses, limited, of course, by the simplicity of the behavior examined and the homogeneity of this high ability class.

1. The product of group thinking is distinctly superior to that of the average and even that of the best member of the group. In this case it is further above the best than the best is above the average.

2. In most cases, with so simple a task, a division among individuals and a summation would give a better product than is obtained by co-operative group discussion. This could not, of course, be applied to the learning that takes place. Individual learning may well be greater when one, as a part of the co-operative group, helps to produce a far larger product than one could when working alone.

3. Groups are more alike than random, or even average, individuals chosen from those groups.

4. The larger the group, within the range from three to ten, here studied, the more superior the group product becomes.

5. Variation in group production depends much more upon how able the best members are than it does upon the ability of those further down in the ability scale.

6. Group production is a matter so different from individual production in the same field that one is practically no index of the other. Most of the factors that make for efficient work as a member of the group, lie outside the range of the things we are doing in education to equip individuals to do tasks by themselves.

READING FOUR[1]

THE NATURE OF HYPNOSIS: AS INDICATED BY THE PRESENCE OR ABSENCE OF POST-HYPNOTIC AMNESIA AND *RAPPORT*[2]

Editor's Note

Reading One emphasized the principle that the social group is an organized whole of which any particular person is a part, and indicated several laws governing the dynamic character of the relationship between parts and wholes. Any whole is an organic phenomenon in the sense that it is an integrated unit, a pattern of energy exhibiting a systematic behavior of its own. The activity of any part within the whole, therefore, occurs in accordance with the laws of the whole. Reading Two demonstrated increased alertness and certain qualitative changes in the thought processes of the individual as a consequence of group influence, thus proving the existence of the dynamic relationship. Reading Three brought out the same fact and also the principle that the whole is more than the sum of its parts.

The present reading pertains to one of the three processes (imitation, suggestion, and sympathy, mentioned in the first reading) that describes the *form* assumed by this dynamic relationship between the individual and the group when actually expressed in behavior. The one selected is suggestion, and the group is reduced to a minimum of two persons. Remember that dynamic relations are expressed in give-and-take processes. Suggestion is a give-and-take process, which means that, in the case of two persons, each must be active toward the other. Professor Young's study of suggestion selects hypnosis as a special case and points to the modes of behavior that are essential to it. Incidentally, the investigation points to the organic character of the situation: two-persons-in-relation. Here, that character is described by the word *rapport*. The two individuals must co-operate. The main theme of the discussion, however, is the fact that co-operation in the trance is limited by purposes which the subject may have had in mind before entering the trance. Heretofore, in the literature on hypnotism, there had been an inclination to overlook the control which the hypnotized person could still exert of his own accord.

For a theory of hypnosis the question whether hypnosis is fundamentally a function of the subject's relation to another or of his relation to himself is of the highest importance. Shall we regard hypnosis as a form of autohypnosis or shall we consider autohypnosis as a kind of indirect hypnosis? Plainly, one's answer to this question determines the direction his theory of hypnosis will take. Just here the difference of opinion between McDougall on the one hand and Ferenczi and Baudouin on the other is clean cut. The first says, "All so-called autosuggestion is in reality heterosuggestion."[3] The last remarks that "Coué would require very little persuasion to induce him to say: 'There is no suggestion, there is only autosuggestion,' "[4] and "The operator's suggestion can be realized solely on condition that it is accepted by the subject's mind and transformed into an autosuggestion."[5]

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[1] Reading Four parallels Chapter III, Wheeler, *The Science of Psychology*.

[2] By Paul Campbell Young, Louisiana State University. From the *Journal of Abnormal and Social Psychology*, Vol. 22, 1928, 372-382. By permission.

The writer has had occasion to refer to this point before. In the first place he reported that four somnambulistic and two other deep-hypnotic subjects with whom extensive experiments were carried on had exhibited, prior to being hypnotized, phenomena of a distinctly autosuggestive nature. For example, one or more of the following traits showed themselves in each of the subjects before he had ever been hypnotized: deep abstraction, reverie amounting almost to ecstasy, putting one's self to sleep at will, actually hypnotizing one's self.[6] In the second place, experiments apparently show that *rapport*, generally considered to be a *sine qua non* of hypnosis, can be present in any degree, or in no degree, depending upon the subject's prior autosuggestions.[7] Most authors report cases of the failure of *rapport* without thereby terminating the hypnotic state.[8] In fact, the autosuggestions of the subject can so far overcome the hypnotist's "moral influence" (McDougall) that the subject can be put into somnambulism by the hypnotist, thereupon be out of *rapport* with the operator, and respond to signals which the subject had decided upon prior to the séance; and can even wake up in spite of the orders being given by the hypnotist.[9]

Although the essentiality of *rapport* is the crux of the question regarding the primacy of autosuggestion as against hetero-

[3] McDougall, W., *Outline of Abnormal Psychology*, 123.
[4] Baudouin, C., *Suggestion and Autosuggestion*, 237.
[5] *Op. cit.*, 240.
[6] Young, P. C., *American Journal of Psychology*, 1926, 37, 356.
[7] Young, P. C., *Psychological Bulletin*, 1926, 23, 511 ff.
[8] Cases cited in note 7.
[9] Young, P. C., *Journal of Abnormal and Social Psychology*, 1927, 22, 130 ff.

suggestion, still the investigation of other hypnotic phenomena can throw light on the matter. Thus, if other hypnotic phenomena can be modified or prevented from appearing by the subject's prior autosuggestions, without destroying the hypnotic state itself, or appreciably interfering with its depth, the presumption in favor of the greater importance of autosuggestion is strong. For, if in a *bona fide* experimental situation the phenomena of a given hypnosis changes in one particular according to the presence or absence of autosuggestion in the subject, there is great likelihood that the other phenomena (at which the autosuggestions of a given séance are not directed) are likewise to be accounted for on the basis of the tacit autosuggestions of the subject. This is about what Baudouin means by saying that "as a rule hypnosis is characterized by increased suggestibility. But it is not necessarily characterized by increased acceptivity."[10] *Acceptivity* here means the tendency to accept another's ideas; *suggestibility,* the tendency to carry out the ideas one has in mind, wherever they come from.

The following data are brought forward as a contribution to the question whether autosuggestion or heterosuggestion is the more fundamental category in hypnosis. This material was obtained at the same time and with the same subjects as that already reported in the *Journal of Abnormal and Social Psychology.*[11] The series of experiments here referred to was planned for the purpose of showing the effects of autosuggestion on hypnotic *rapport.* However, after the experiments were finished and the results studied, it became evident that the subjects' autosuggestions had modified not only the nature of the hypnotic *rapport* but also the extent of the post-hypnotic amnesia.

The conditions under which the data were obtained were quite simple. Three Louisiana State University students, chosen from among several who volunteered, because they could easily and consistently go into the somnambulistic state, were asked by the experimenter to give themselves autosuggestions to see whether they could thus alter their hypnotic behavior. In all experiments, except the first two of subjects H. and R., a colleague of the writer coached the subjects in elaborating their autosuggestions. What effect the autosuggestions had on the *rapport* between the subjects and the experimenter has already been reported.[12] The rest of this paper will show the effects of the autosuggestions on the post-hypnotic amnesia, that was always ordered by the operator during the hypnotic séance.

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[10] *Op. cit.,* 306.

[11] See note 9.

Subject H.—Before this subject tried any autosuggestions on himself he showed the usual post-hypnotic phenomena. It is the writer's opinion that in this case, as in the other cases of amnesia following the hypnotic state, the amnesia results from the fact that the subject comes to be hypnotized with the idea in mind that he is to forget what takes place in hypnosis. Or if his conception of hypnosis does not involve forgetting what takes place in the trance state, the subject gets the idea from the whole tenor of the suggestions, even if he is not told, in so many words, to forget.

Experiment 1.—The first autosuggestion the subject gave himself was as follows: "While in a state of hypnosis I will obey all commands except that of becoming insensitive to pain." The séance, like all the others to be referred to in this paper, consisted of manifestations of the classical hypnotic phenomena: auditory and visual hallucinations, delusions, hypnotic amnesia, functional paralysis, analgesia, etc. All of these phenomena appeared in this session except analgesia. The subject's complete record of his memories of the hypnotic session, made, of course, after the session was over, is as follows: "I have no definite remembrance of anything after being put in the chair. I have a vague remembrance of some object hitting my hand a few times. It seemed as though I was told that I was not to feel it, but I was fighting that order and was straining my senses to feel it. The object seemed like a pencil point, rather used and blunted. It was not painful in the least." As a matter of fact, the subject in hypnosis had acted as if the prick of the pin was very painful. There was a pencil such as the subject referred to lying on the table near by when he awoke. The interesting thing about this record is that the only memory for the events of a long séance was connected with the autosuggestion that the subject had given himself. Even though this detail was vaguely remembered, it seems to show that the determination, prior to hypnosis, not to co-operate on one point alters the hypnotic consciousness in some way, without necessarily interfering with its depth. There was amnesia for those happenings in hypnosis toward which he had assumed an attitude of indifference; memory of a kind for that event in which he had interested himself. It is to be kept in mind that the operator had enjoined amnesia. It seems from this experiment that post-hypnotic amnesia is a matter of the subject's tacit autosuggestion that he will not remember.

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[12] See note 9.

Experiment 2.—At the suggestion of a colleague of the writer H., the subject, gave himself the following autosuggestion: "While in a hypnotic state I will obey any and all commands except that I forget everything or anything that I do or that goes on." The result of this attitude assumed before being hypnotized was that the subject was able to recall almost everything that went on during the séance, the experimenter's hypnotic suggestions to the contrary notwithstanding. Part of his long report after awaking is appended here: "Everything in this séance seemed clear after being awakened; whereas I can scarcely remember anything of the just previous one." The action of the subject in hypnosis and his statement afterward incline the writer to believe that the genuineness of a given hypnotic session, the events of which are remembered in pursuance of the subject's pre-hypnotic determination to remember them, is no more questionable than the genuineness of a séance the events of which are remembered because the operator during the séance suggests to the subject to remember them. If this is the fact, amnesia may depend on the attitude of the subject, regardless of whether that attitude is self-assumed or assumed at the operator's request, or simply because it seems the best way of co-operating in the experiment.

Experiment 3.—H. this time before coming to the séance had written his autosuggestion (elaborated with the help of the university professor), as usual, on a sheet of paper, which he kept in his pocket until after the session was over. After the experimenter had gone through the séance, eliciting the whole repertoire of actions already referred to, he opened the paper and read this autosuggestion: "I will submit to hypnosis normally. During hypnosis I am to retain my sense of color; and if shown a blue card I will write 'Peabody Hall, May 4, 1926,' on it. If it is any other color I will write 'Failed' on it." It is not to the point here to show how far this autosuggestion was carried out; the important thing for our purpose is that the post-hypnotic report of the subject showed no memories except those connected with his autosuggestion. This is the report in full: "I subjected myself to hypnosis as agreed on the card, bearing the autosuggestion on it in mind. I was shown two cards—one blue with dark brown printing on both sides and one tan (light). I was vaguely aware that I should do something to each, but could not recall what it was. That is all I can remember, except that I have a very faint idea or remembrance of seeing some large city. The buildings seemed dun-color and built of brick.'' While the experimenter was trying vainly to make the subject ''take'' an hallucination of a blue card as white, he had easily brought about the hallucination of a large city, which the subject said was New Orleans. In this way the memory of the hallucinated city became connected with the autosuggestion. That these two items were the only memories out of a long session, that they were retained in spite of the experimenter's hypnotic command to forget, lends color to the theory that autosuggestion may be more powerful than heterosuggestion.

Experiment 4.—''While in the hands of the experimenter I will do anything suggested to me up to and including the third suggestion. After that I will respond to none except the command to wake up.'' This autosuggestion was carried out in spite of all the operator could do. Nevertheless, the subject reported on waking that he had really been hypnotized and had been acting mechanically. The manner in which he behaved in hypnosis and the nature of the post-hypnotic amnesia bears this out. His memory for the first three items was pretty clear; for all the rest it was hazy. For completeness, the whole post-hypnotic report of the subject is given. ''The first stunt I was called upon to perform was turning my arm around in circles. This I did. The second was stiffening the arm—also worked. The third was lifting my feet. I was then told to do or not do certain things—rather that I could or could not do certain things. The suggestion had no effect. I was given a post-waking suggestion, but paid no attention to it—something about some ink. I was told to forget everything until next séance when I am to tell it. I was waked up by a count of five. Before 5 I was told to write something, but did not seem to care to do so, and did not. That is all I remember except that my arm is still sore from having it pulled off.'' After it was apparent that the subject had amnesia for the definite events after the third ''stunt,'' he was told what he had done. Thereupon he added to his report as follows: ''Everything after the third suggestion was just like something holding me down. Just as if you were telling me I could not, and I said, 'He doesn't know what he is talking about.' After

I took a step or two, I could walk easily. The post-hypnotic suggestion was something about some ink; just did not pay any attention to it, did not matter whether I heard it or not; not paying attention to anything; just lazy, half asleep. Before I went under I had given myself no autosuggestion in regard to remembering. Things are not so clear as when I suggested to myself before that I would remember.'' In response to questions he said that he had felt insensitive to pain [In reality he had acted as if he were suffering from the pin-pricks], that when he was put on his feet by the operator he ''couldn't stand and felt as weak as a kitten.'' In reply to a question, the subject stated that he had not been conscious of counting, but that he just knew when three had passed. It is noteworthy that the attention which he had had to give in order to keep account of the first three items made his post-hypnotic memory for them much more definite than for anything else that had taken place during the whole séance. Here, again, the self-assumed attitude of the subject seems determinative not only for the amount of *rapport* but also for the extent of post-hypnotic amnesia.

Subject R.—This student, like the other two in these experiments, characteristically showed so-called total post-hypnotic amnesia, even without a specific order in hypnosis to forget. That this was simply temporary loss of recall; and that, indeed, there is no total amnesia following hypnosis is the writer's conviction.[13]

Experiment 1.—For R's first attempt to pit his own suggestions against those given during the hypnotic séance, he chose this simple one: ''I can open my eyes.'' And so he did, even though he was distinctly told that he could not; but that is not the important thing. What concerns us now is the fact that the only memories of a long session in which the subject had been kept busy doing many different kinds of tasks, was to the effect that he had opened his eyes and had seen dogs in the corner of the room. The dogs were pure hallucinations; but his post-hypnotic memories were limited to matters about which he had given himself autosuggestions.

Experiment 2.—In carrying out the autosuggestion determined upon before this experimental session, the subject showed, in his case, at least, the superiority of autosuggestion over heterosuggestion. His self-suggestion was: ''I will do everything I am told except I will be able to walk, and if told not to, I will remember everything that happens.'' His complete post-hypnotic record is as follows: ''Looking at penny, moving my arm in a circle, trying to bend my arm, walking, asking me if I felt something, trying to uncross my fingers, trying to think of my name, seeing a bird, thinking of where I was, trying to open my eyes. I remember more than before, but things seemed blank every time you asked me. When I first began to walk, I walked naturally. I could not think of my eyes as I could think of my feet. What happened then took me a long time to think. Could not concentrate then as can now. [In response to a question]: Seems more like sleep, than waking—in between them—a dull state, like some dreams. It affects me just like a dream except in dreams one can't remember.'' In answer to the question why he had responded in hypnosis that he was at home, the subject said: ''I felt as if I was at home; felt free and nothing to trouble me or bother me.'' As usual, the operator had enjoined post-hypnotic amnesia. Since in this case the memory induced by autosuggestion embraced every item except the induced analgesia, and that palpably without interfering with the genuineness of the hypnotic state, it seems likely that autosuggestion may be more powerful than heterosuggestion in determining the phenomena of a given hypnotic séance.

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[13] See note 7.

Experiment 3.—This experiment furnishes an excellent example of the power of autosuggestion in overcoming heterosuggestion. It shows no less clearly the tendency of memory to cling to one's definite autosuggestions. The subject had written down and said over to himself several times before coming to the experimental room these directions: ''I will submit to hypnosis. When fully hypnotized I will not respond to suggestions from the operator. But will respond as follows to A.[14] When A. says one, I will raise my right arm shoulder high. When A. says two, I will relax right arm. When A. says three, I will stand. When A. says four, I will about face. When A. says five, I will sit down. I will awake when three taps are given by A.'' All the subject could remember after waking is this: ''Looking at the coin; raising my arm at the count of one; then lowering it at the count of two; standing up at the count of three; turning around at four; sitting down at five; three taps made me wake up.'' In response to questions his answers were: ''I was hypnotized; felt just like other times. When I came in I just had the suggestion on my mind, and kept it there during hypnosis. There was no difficulty in knowing what the signals meant. I remember the experimenter's talking to me just when he held the coin up [as a means of inducing hypnosis]. I do remember A.'s voice.'' Here we have amnesia of a very complete kind for all the orders of the operator during the whole séance and memory for only the voice and the signals given in accordance with the subject's prior determination. It is unlikely that this memory is merely a hang-over from the discussion he had had with A. before the hypnotic session, because the subject not only remembered the things that he did in hypnosis, but remembered having done them.

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[14] The writer is indebted to Professor I. P. Foote of Louisiana State University, herein referred to as A., for elaborating the autosuggestions the subjects gave themselves in most of these experiments, and for taking a complete record of the proceedings.

Subject G.—Experiment 1.—''At the suggestion of the experimenter, I felt quite comfortable and eventually lost consciousness. He told me that my feet were glued to the floor and that I couldn't lift them. I knew very well what I was trying to do, but I couldn't find my feet. He also did something to my arm that caused me to lose it. At length I heard a tap and knew that I must rise; at two taps, I knew that I must walk, and three taps, I had to stop. Some one made a very loud noise which caused me to waken, and it seemed to frighten me for just an instant.'' Such was the extent of the subject's spontaneous post-hypnotic recall. In answer to questioning, the subject made the following additional statements: ''At the time I heard the tap I just had the impulse to rise; I don't remember what the experimenter was saying while I was trying to rise—don't remember anything the experimenter said after taps began. I am sure I did not hear, if he did say anything; do not remember trying to open my eyes, nor anything about my hands. It was really light sleep, like dreaming. I had no idea where I was when I woke up.'' The subject's autosuggestion was that he would respond to the signals as he did in fact, that he would respond to no orders of the operator after going to sleep and that he would wake up at a loud noise. His memory for the two items that preceded the first signal seems to be a clear case of an expectant attitude's coloring them with interest. When the signals began, however, the predetermined attitude of not paying any attention to anything else was established, accounting for the memory of his reacting to these signals and the amnesia for everything that the experimenter said and did to him. The reality of this hypnosis is attested not only by the action in hypnosis (his analgesia, his manner of being in a trance) and his own post-hypnotic statement, but also by the behavior at the moment of awaking. It was quite evident that he had been dreaming and that the loud sound that aroused him had a lively part in the dream. He was so disturbed for a few moments after awaking that he had to be restrained. These reactions are in strong contrast with the lack of response to the experimenter's suggestion after the signals began to sound, and the amnesia for all that the experimenter had said after that time.

Experiment 2.—G. at the instigation of the writer's colleague gave himself this plan for an autosuggestion: "I will go to sleep at the will of the experimenter. I will co-operate with him until A. raps twice. I will then not follow suggestions of the experimenter, but will in order do the following without signals: 1. Raise right arm horizontally. 2. Let right arm fall. 3. Tap with right foot three times. 4. Extend left foot. On three taps from A. I will resume *rapport* with the experimenter." The subject's complete record after the hypnotic session had been concluded is as follows: "At the suggestion of the experimenter I went to sleep. After that I drew some circles with my arm, had my arm stiffened, and my feet became glued to the floor. I was conscious of what he was doing and telling me, until suddenly I heard two taps and then I lost consciousness of him. I then felt an impulse to raise my right arm and let it fall again. Then I wanted to tap my right foot, and after tapping it three times I no longer wanted to tap it. My left leg then felt that it should be stretched out, and I remember stretching it out. The next thing I knew I heard three taps, and the experimenter was by my chair again. He had me drawing circles with my left arm, made my fingers clench in a tight grasp, made me get up and walk; and while I was walking he told me that my feet were glued to the floor, and it seemed that they were. He then told me that I would wake up when he counted to the number five, and before he counted that far I could feel myself taking on more life. Then when he said five, I felt fully awake." On being catechized, the subject said that he had given himself no autosuggestion as to remembering, that he remembered nothing else [At the time of giving the suggestion of negative auditory hallucination the experimenter had let a table fall to the floor with a terrific crash], that the series of taps and appropriate mummery had been gone through only once. In reality the whole dumb show had been enacted twice.

The results of this very complicated session seem to point to the hypothesis that post-hypnotic amnesia does not inhere in somnambulism, nor in the suggestions of the hypnotist *per se,* but in the overt or tacit autosuggestions on the part of the subject. When, for some cause or another, the attention of the subject is directed toward his own performance in hypnosis, his memory for what took place in the séance becomes very much more extensive.

SUMMARY AND CONCLUSIONS

1. This paper reports a series of experiments planned for the purpose of testing the essentiality of *rapport* in somnambulism.[15] The results show that in the case of three somnambulistic subjects post-hypnotic amnesia, as well as *rapport,* can be greatly modified or eliminated without destroying the hypnotic state.

2. That there is never total post-hypnotic amnesia was stated long ago by Bernheim and given definite verification in a series of experiments made by the present writer.[16] It now seems likely that the presence or absence of post-hypnotic amnesia depends on the overt or tacit autosuggestions of the subject and not on the depth of the hypnosis as such.

3. Post-hypnotic memory may exist at the subject's prior autosuggestion, in spite of the operator's explicit hypnotic command to forget.

4. Generalizing, it is possible that Bernheim's *cliché,* "There is no hypnosis, there is only suggestion," may be accepted in the sense that there is no phenomenal *sine qua non* of hypnosis. Hypnosis may be modified in many particulars without varying appreciably its depth. Moreover, hypnosis will take the form that the subject to be hypnotized has been led to believe it will take. Since, usually, the form the subject expects corresponds with the form that the hypnotist has been accustomed to regard as *bona fide* (of course from his reading of books and seeing demonstrations; which information has, likewise, filtered down in diluted form to the subject), there has come about a tradition that certain phenomena are symptomatic of somnambulistic hypnosis.

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[15] Already published; see note 9.

[16] See note 6.

5. When, rarely, as with whirling dervishes, Hindu fakirs, susceptible persons at saints' shrines, and normal subjects in an experimental setting, the person's expectations differ from those detailed in books on hypnosis and broadcast by all those who hypnotize, the subject may show only a few, or none, of the classical hypnotic phenomena, and still maintain, the present writer thinks with reason, that he had been in a trance state.

6. Thus, in cases where the firmly-set expectations of the subject, whether it be long-held moral or other convictions or merely autosuggestions given to himself prior to a certain séance, differ from the commands of the operator in hypnosis, with normal subjects the autosuggestions will likely prevail.

7. It is clear from what has been said that in the opinion of the writer the results of this series of experiments, and general considerations as well, help to substantiate the theory that the essential element in hypnosis is the autosuggestion of the subject, rather than the heterosuggestion of the operator. Heterosuggestion is effective only because, and insofar as, the subject has overtly or tacitly given himself the autosuggestion that what the hypnotist says will take place. This autosuggestion may be in the form of belief in the power of the operator; it will more likely be in the form of a willingness to co-operate with him.

GLOSSARY

Amnesia, loss of memory; may be partial and temporary or relatively complete and permanent, depending upon conditions.

Analgesia, insensitivity to pain.

Auditory, pertaining to the sense of hearing.

Autosuggestion, suggestion a person induces in himself.

Delusion, false belief or idea about something accepted as real or true.

Functional Paralysis, paralysis of the muscles not induced by organic disturbances such as occur in injury and disease; a paralysis induced in the course of suggestion and removed in a similar fashion.

Hallucination, "false" awareness of an object or event when the inciting causes are exclusive of the object or event itself.

Heterosuggestion, suggestion induced by another person.

Hypnotic Trance, a limited mode of conscious behavior brought about by a co-operative restriction of controlling conditions; a mode of behavior resembling an exaggerated day-dream.

Illusion, misperception of an object actually present, or an event actually taking place.

Phenomenal, descriptive; apparent.

Post-hypnotic, referring to a period of time subsequent to a given trance; post-hypnotic suggestion is suggestion carried out subsequent to the trance but as a consequence of conditions induced by the hypnotizer at the time of the trance.

Sine qua non, "without which, nothing"; an essential feature of something; criterion.

SOMNAMBULISTIC STATE, hypnotic and certain other trances of which the subject is subsequently unaware; a limited state dissociated from normal waking life; sleep walking a special case.

GROUP II

INTELLIGENT BEHAVIOR

READING FIVE[1]

TAKING THE DOGMA OUT OF THE I.Q.[2]

Editor's Note

Pursuing the plan outlined at the beginning of Reading One, the conditions of human behavior are now to be considered to some extent restricted. The behavior that results from these conditions will be proportionately limited. Recall that the chief and constant feature of the behavior studied so far has been its social character, and, as restriction of conditions is commenced, note that the social aspect suffers first. Mores, folkways, and imitation are left behind. But, while these aspects recede farther into the background as more and more limited modes of behavior are studied, no amount of limitation removes them entirely. From among the remaining conditions those have been chosen that bring out a less complicated aspect of behavior, namely, its intelligent character. These conditions are: (1) the problem-character of the stimulus-situation, a situation that may still be social, but involves *essentially* the establishing of a goal, or remote end, before the response commences; (2) a nervous system is sufficiently well developed to make possible an organized or configurational response to the total situation.

In the light of the principles set forth in the previous readings it can be predicted that the intelligence of the individual is within limits under the control of the social group. The opposite view is still held in certain quarters, that intelligence is exclusively a matter of native endowment. This view is based upon the observed constancy of the I.Q. in the same individual over a period of years. An I.Q., or intelligence quotient, is the ratio of a person's (preferably a child's) performance, in a given test, to the average performance of a large group having the same chronological or physical age. A child's performance of 10 per cent. above the average fixes his I.Q. at 110; a performance at the average means an I.Q. of 100; a performance of only 50 per cent. of the average signifies an I.Q. of 50, and so on. The theory of the constant I.Q. presupposes, for example, that a child six years of age with an I.Q. of 70 may develop mentally after that time, but when twelve years of age will still have an I.Q. of 70. The basis of the constancy was said to be inheritance. It now seems likely that whenever an I.Q. remains constant, as it often does, it means only that the conditions under which the child is developing remain constant. Recent studies show that, when conditions change, the I.Q. changes, and that social factors should be included among these conditions. Dr. Cornell's paper indicates the importance of these social influences in the mental development of a child with a low I.Q. It is not to be concluded, however, that inheritance, as ordinarily defined, does not play a part.

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[1] Reading Five parallels Chapter IV, Wheeler, *The Science of Psychology.*

[2] By Ethel L. Cornell, Psychologist, New York State Department of Education. From *Mental Hygiene,* Vol. 11, 1927, No. 4, 804-810. By permission.

The "constancy of the I.Q." has been hailed as a great boon for education, since it enables us to predict a child's future accomplishment from his present status in an intelligence test. The idea has met with some opposition, but the controversy has for the most part subsided, the honors going chiefly to the holders of the doctrine. To the victor belong the spoils, it is said, but in this case there has been a sort of boomerang reaction, which has made the spoils rather unpalatable. That is, the statistical fact, which it may be said the victors have proved, is that on the average the I.Q. has not been found to vary more than 5 points plus or minus from one test to another, when standard conditions of testing are maintained. This has been taken by lay school people—and by many enlightened school people, it is to be observed—to mean that, no matter by whom obtained, no matter what the conditions of testing, no matter even what qualifying statements may be made by the examiner in reporting the examination, an I.Q. may be regarded as a definite and final label, not "on the average," but for each individual considered.

The writer recently said to a school superintendent, in discussing a boy who had just been examined, that while he had now a very low I.Q. and was certainly mentally inefficient at the present time, it was her opinion that much of this was due to his marked feeling of inferiority, and that if the school could stimulate him and encourage him and give him more self-confidence, a later test might reveal a higher I.Q.

"Higher I.Q.?" queried the superintendent in surprise. "I thought that wasn't supposed to change."

The layman, however intelligent he may be, is very apt to take the statistical fact of an average result for the invariable fact of common sense. When common sense says: "It is usually so," it means that it is so about 95 per cent. of the time; and this is the "apperceptive mass" or the common-sense concept to which the "average statistical fact" is commonly assimilated.

At the present time enlightened school superintendents, principals, and teachers are reading psychological and scientific journals as never before, and are getting ideas from research studies, which they inevitably assimilate in accord with their particular background of knowledge. It seems, therefore, as if research workers should issue more frequent cautions as to the common-sense application of their findings. Instead, however, I believe that psychologists themselves have done some propaganda work along this line which they should not be proud to look back on. To be specific, I believe that the present tendency of the schools to encourage persons, after a brief course in testing, to give individual intelligence tests to children who are school problems is on the whole to be deplored. And I believe that the tendency of psychiatric children's clinics to employ 'psychometric examiners' of meager training is even worse, since the public expects the clinic to render expert service. My reason for believing this tendency to be bad is not because there is anything abstruse about the technique of administering tests or anything extraordinarily difficult about the scoring that cannot be learned in a short time by an intelligent adult. The reason is, rather, that without a wide background of psychological knowledge, as well as an extended period of supervised practice in testing, it is not possible to interpret a test adequately. If in 50 per cent. of the cases the I.Q. will remain constant, then in any given child who is examined, the chances are 1 to 1 that it will *not* be constant. The I.Q. itself, however accurately the test may have been given and scored, is not enough to tell us on which side of the fence this particular case lies. The inexpert examiner has not the knowledge to help us much, and, indeed, it is always emphatically proclaimed by the advocates of "short-horn" testing courses that such an examiner is not expected to interpret. Of what avail, then, to test? If an I.Q. can be taken at its face value, anybody intelligent enough to give a test can interpret it. If it cannot, then it is of little practical use uninterpreted.

The I.Q. reminds me of a certain type of puzzle picture, familiar to children's pages in newspapers a number of years ago, which, when first regarded, appears as an ordinary scene of a house and garden or what not, but contains, hidden in the lines of shrubbery or roof or clouds, the outline of a face or figure. It may require diligent searching and concentrated disregard of the ordinary "meaning" into which the lines of the picture are automatically fused before one can find the concealed figure. But when this is once discovered, it is amazing how it dominates the picture. Look away from the picture to regain your orientation; look back again and the concealed part still stares out at you in bold relief. The rest of the picture pales into a mere blurred background and the meaning of the picture as a whole is lost.

There is some danger that in interpreting the picture of a child's personality we may find that the intelligence quotient assumes something of the character of such a concealed figure. Though it has been a part of the total picture always, we have only recently discovered it, and it has sometimes stood out with the dominance of the concealed figure of the puzzle picture, so that the meaning of the whole has been overlooked. Of course the analogy breaks down if one tries to carry it further, since the hidden figure is entirely extraneous to the interpretation of the picture, while the I.Q. is an intrinsic part of the individual's personality picture.

Dr. Fernald taught us—at least in dealing with the feebleminded—not to let mental age and I.Q. alone assume undue importance in our estimate of an individual. We all recognize the importance of considering other factors, such as family history, personal development, and social, moral and economic status, before making a diagnosis. But there is another step necessary, which we have not always recognized. We must know whether the mental age and the I.Q. are valid measures of the native intellectual ability of an individual, since that is what they are conceived to be. This we can tell only by a psychological interpretation of the test. Having found the puzzle part of the picture, we must take it apart again and fit it into the whole.

These are really two issues—the question of diagnosis and the question of psychological interpretation. Nevertheless, they are often confused. We may illustrate the first point by supposing two adults, each of whom has a mental age of 11 years and, by Terman's standard, an I.Q. of 70.

Suppose that this represents in each case a valid measure of their ability to deal with the tasks of the Stanford-Binet scale. One of these individuals may show so lamentable a lack of success in attending to his affairs that no one would question the diagnosis of feeblemindedness; the other may live his life peaceably in the community without ever being suspected of needing any supervision. Both have the same mental capacity, but their adaptation to society depends on other factors. These two cases represent the first issue, in which aspects other than mental ability determine the diagnosis. This is perhaps rather a legal or sociological question than a psychological one.

The second issue is a strictly psychological one, and psychologists have by no means always made it clear in their interpretation of their findings to the psychological layman. (May it be added, parenthetically, that medical men, school administrators, and teachers are more frequently than not "psychological laymen" in this respect?) A mental age has been taken at its face value and the resultant I.Q. used as a basis of prognosis with far too few precautions. Given a child who does not refuse to answer questions, an examiner who knows the technique, and a certain allowance for the "probable error" of the measurement, and the average teacher, school superintendent, and physician, who have some acquaintance with mental tests, will accept the mental age and I.Q. without further question. The second point—the necessity of interpretation of the I.Q. itself—may be illustrated by the following story of a little boy.

This little boy, whom we may call John for convenience, was examined with a group of other children selected because of their slow school progress as possible candidates for a special class. He had spent three terms each in the kindergarten, first and second grades, and was an unsatisfactory pupil in the third grade when he was examined, just before he was ten years old. He had, on the Stanford-Binet scale, an I.Q. of 78, which seemed, to a rather hurried examiner, quite reasonable. True, a series of performance tests at the same time gave him a mental age within two months of his chronological age, which this examiner attributed offhand to better ability to deal with concrete things than with abstract. However, an I.Q. of 78 seemed a little high for a special class, so the examiner hunted around for a little more light on the subject. A rather unexpected bit of evidence came to hand. This little boy, during his sojourn in kindergarten, had had two mental tests—one, the Stanford-Binet, given by an examiner whose knowledge of the technique could not be questioned; the other, the Pintner-Cunningham group test. On these two tests, his I.Q.'s were respectively 104 and 101. What shall one say to this: three tests with average scores against his present Binet rating of 78 and his continued failure in school? Shall one dismiss it as just an extreme case of disagreement between tests, or may one find it an opportunity, by an analysis of the psychological facts, for a better understanding of the child?

Perhaps the analysis of a test is not an exciting or highly entertaining procedure, but there is a certain thrill in discovery that makes an explorer endure hardships without complaint. Will you explore with me?

Two tendencies were apparent upon an analysis of John's present Binet test. One tendency was to begin an answer or a response to a certain task with an adequate understanding of it, but to lose the thread of it and trail off to something irrelevant that failed to satisfy the demands of the task. The other tendency was to continue a train of thought started in response to one situation when a different situation was presented, a tendency technically called *perseveration.* Both tendencies show a failure to control his associations as easily as a normal child should. But this is an abnormal rather than a subnormal matter—not enough so, yet, to show itself in any crazy actions or to make his associates think him queer. But the tendency was apparent even in the earlier tests and was more marked in the later one. It was present also in the performance tests, though less evident than in the Binet—doubtless because the concrete material served to keep the problem before him more specifically. Moreover, when he was six, he had the vocabulary of an average eight-year-old, and when he was not quite ten, the vocabulary of an average twelve-year-old. Besides, he put together without effort the dissected sentences which constitute one of the hardest of the twelve-year tests. And the examiner had been moved to attribute his superior score on performance tests to a lack of facility to deal with tests involving linguistic abstractions! One rarely finds retarded children whose knowledge of word meanings is even average for their chronological age, unless they come from homes of superior culture and have had much special training. These are the psychological facts. The only other factors known about him are that he is an only child, that he has been absent much because of an undefined "nervousness," and that he is shy and timid, although not unfriendly, in school.

How, then, does an I.Q. of 78 fit into the perspective of this picture? I venture to put it this way:

John is an only child who started school with some of the repressions common to only children because they have been compelled to adjust themselves overmuch to adult modes of behavior. Either because of this, or because of an inherent nervous instability, John is recognized as a nervous child to an extent serious enough to keep him some time out of school. John has not learned to play with other children, and he finds it easier to stay on the outside than to go through the painful process of adjustment. Among 40 kindergartners, he is not singled out as remarkable, but only as slow, and so he stays in the kindergarten a second term and yet a third. Each new group of children makes him feel a little more isolated, a little more inferior, a little more discouraged and uncertain of himself, and he reaches the first grade at last without the self-confidence that is so real a spur to success. This process is repeated so that John is more or less confirmed in failure. He doesn't entirely understand it, he becomes more timid, more reserved, more unconsciously preoccupied with his little problem, so that he does not react freely and naturally to a mental problem. His pre-school nervous tendencies become more marked.

The stage is set for one of the innumerable tragedies that might be averted. John may continue to develop abnormal mental trends until he reaches the state hospital; or he may stop short of that and be only eccentric and apparently dull; or failure may become so habitual that he will give up the problem and take himself at other people's estimate of him, becoming, so far as any mental efficiency is concerned, actually subnormal.

If John's friends can be made to believe in him and to convey their belief to John, if wise guidance can be had to bring him out of his timidity and spur him on to take his part normally in social contacts, if he can have the stimulus of success to cheer him on his way, then John may be saved from the mental wreckage of the community ten or twenty years hence.

Is it not worth while to spend the time and effort necessary to get all the facts, including the low I.Q., into proper perspective, and *after that to see that something is done about it?*

GLOSSARY

Apperceptive Mass, background of experience.

Group Test, usually means an intelligence test so devised that it can be given to groups of individuals simultaneously.

Psychiatric, pertaining to a study of nervous and mental disorders and maladjustments.

Psychometric, pertaining to the measurement of different aspects of mental life such as intelligence.

Stanford-Binet Test, a standard intelligence test originally devised by two French investigators, Binet, and Simon; revised and adapted for use in this country by Professor Terman of Stanford University.

READING SIX[1]

INTELLIGENCE AS METHOD OF ADAPTATION[2]

Editor's Note

The 27th Yearbook of the National Society for the Study of Education[3] contains an extraordinary collection of researches designed to reveal the relative importance of inheritance and environment for mental development. Outstanding among these contributions were two studies, one made at the University of Chicago[4] and the other at Stanford University.[5] These investigations stimulated much discussion especially over interpretation of results. Professor Terman and his associates at Stanford University have been inclined to emphasize inheritance as the cause of constancy in the I.Q., while Professor Judd and his associates at the University of Chicago have stressed to a greater extent the importance of a constant social environment. Many people have interpreted the earlier Stanford researches to mean that the I.Q. is so fixed by heredity that nothing can change it, obviously an unscientific position. Accordingly, those who have stressed the other side of the problem heartily welcomed an admission from Stanford that, under the right conditions, environment does modify the I.Q., at least to a slight extent. The corresponding study from the University of Chicago indicated that social influences were several times more important than the opposing investigation showed. In the following article Professor Judd argues for the importance of environment and hence implicitly assumes an organismic position with respect to the entire problem.

The most impressive fact with regard to this *Yearbook* seems to me to be that the two major investigations, one conducted at Stanford and the other at Chicago, resulted in an agreement. To quote Miss Burks' statement made on page 318, "The investigations agree in attributing small, but significant increments of I.Q. to superior environment." Such a pronouncement made by a representative of Stanford cannot fail to effect profoundly the thinking of the educational world. It has been commonly believed that the Stanford group regards education as the exclusive right of those whom heredity has made competent and that intelligence is a product of heredity. If it is now agreed by Stanford that a superior environment can produce a positive change in the I.Q., the educational program of the country can, with full assurance, continue its effort to provide a superior environment for everybody.

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[1] Reading Six parallels Chapter IV, Wheeler, *The Science of Psychology.*

[2] By Charles H. Judd, University of Chicago. From the *Journal of Educational Psychology*, Vol. 19, 1928, 397-404. By permission.

[3] Bloomington, Illinois Public School Publishing Company, 1928.

[4] Freeman, Holzinger, *et al.*, *27th Yearbook*, Part I, 103-218. *Cf.* Wheeler, *The Science of Psychology*, 108 f.

[5] Barbara S. Burks, *ibid.*, 219-316. *Cf. The Science of Psychology*, 109.

There are many of us who have felt clear that there is a demand in America for the best possible environment for every child and we have watched with some anxiety the spread of what appeared to be a fatalistic theory among those who have adopted without reservations the view that the I.Q. never changes. Now that the leading investigators are all agreed that a superior environment improves the I.Q., we can organize the schools without any of the inhibitions which arise from a false view regarding the absolute fixity of the I.Q.

There remain, of course, a number of problems on which there is still lack of complete harmony. For example, it is difficult to find any real justification for the effort of the Stanford investigation to put into numerical terms the relative importance of heredity and environment. In her summary on page 308, Miss Burks ventures the statement that, "on an average, less than ten points of I.Q. must be due to environment." The figure "ten points" is derived from a series of calculations which throw together data that are so heterogeneous in origin that they might better have been, in my judgment, thought of as relatively loosely related until the discussion can be conducted on a much more mature basis. Furthermore, I find this kind of effort at quantitative determination likely to be altogether misleading. Suppose that it ultimately can be shown with infallible certainty that a superior environment is capable of increasing the I.Q. by ten points, what does this mean? I think of a mechanical analogy. An engine is drawing a train at the rate of forty miles an hour and by the use of more fuel and better methods of operation the speed is increased by ten points. Are these last ten points like the first or second units of ten? Or consider, if you prefer, the engine's start. When it pulls the train from zero speed to ten miles an hour, is the achievement the same as when this same engine adds ten miles speed to the first ten and so reaches twenty miles an hour? The mechanical analogy may not be perfect but it expresses in a concrete way the objection which I find to the statement that heredity supplies seventy per cent. or eighty per cent. of intelligence and environment supplies ten to twenty per cent. If the educational environment, when properly organized, can in a single lifetime speed up intellectual adaptation to a degree equal to one-seventh or one-eighth of that which is supplied by the long processes of human and animal evolution, we may well think of environment as of stupendous importance.

A second point at which disagreement appears is as follows. Discussion in the past, and to some extent in the present, seems to me to be obscured by the overemphasis which the hereditarians have given to the physical transmission of traits through the bodily organism. It is certainly a matter of major importance to human life that the normal individual comes into the world with eyes, ears, and vocal cords and of finger muscles which are capable, after a time, of wielding tools and of forming the letters of the alphabet. No one will deny the importance of these physical traits. There is in the world, however, a freightage of the first importance to human life which the bodily organism cannot carry. Physical heredity is impotent to transmit language or science or art. Evolution of the type which is essential to the perpetuation of civilization requires the handing down from generation to generation of literature and number and governmental policies. The germ plasm cannot carry these and, therefore, it has been necessary in the course of the world's progress for channels of transmission other than those of physical heredity to be devised and conserved. You and I did not inherit the English language through physical transmission of traits. We have, however, since the date of birth acquired, in addition to the powers which nature gave us through physical heredity, certain modes of behavior which have made us what we are as civilized human beings. We have passed, since we were born, through a series of experiences which are quite as important in determining our thinking of tomorrow and the next day as are any of the experiences through which the germ plasm passed before birth. We have inherited much from the efforts of those who through long decades have perfected and recorded the English language and have thus given to each of us an instrument for the acquisition and expression of ideas which is superior to any device of communication that has ever appeared in the history of the animal world. Language is transmitted, not through germ plasm but by a method of evolution which produces adaptations utterly beyond the reach of germ-plasm transmission.

The effort to lump together physical heredity and such devices of adaptation as language, and number, and science leads to endless confusion. Language is an acquired form of skill. Why should any one overlook language in cataloging the inheritances which come to us from our ancestors? To be sure, language comes to each one of us by a process other than that by which we inherit eyes, and ears, and vocal cords, and hand muscles. To be sure, languages in different parts of the world differ more than do the physical traits of human beings, but that only goes to show that language is a highly complicated function which physical heredity cannot carry. What physical heredity cannot carry must be transmitted by some other channel. Handwriting, like spoken language, was never inherited by any human being and yet language and writing have transformed human existence. They can properly be described as fundamental methods of civilization. In like fashion, we may catalog a long list of methods of thought and investigation and we may properly say of them that they are products of social inheritance.

Let us put the matter in terms of a concrete comparison. Suppose that a member of modern society had to choose between, on the one hand, an I.Q. of ninety plus such knowledge as he can absorb of all that modern science can teach and, on the other hand, an I.Q. of one hundred and such limited knowledge as Robinson Crusoe and his man Friday could in their isolation accumulate without the help of the teachings of science, which would he choose as more likely to contribute to a successful career?

I did not institute the comparison which I have just suggested without full realization that the extreme advocates of the absolute fixity of the I.Q. provide themselves with a way of escape by saying that intelligence is the capacity for taking from the environment all that is desirable. Their very definition of intelligence, therefore, absorbs the environment into what they call intelligence. They then turn on environment and call it a matter of very minor importance. They admit that it requires intelligence to absorb science but they seem to overlook the fact that the processes by which science has been produced are manifestations of intelligence and that science is the carrier of devices of adaptation which have completely transformed the natural environment. Science is certainly something different from the natural environment. Why should we not recognize that it is a body of methods of adaptation which, when transmitted, constitute a part of the individual's equipment no less than do his physical traits? The extreme hereditarians commit the fallacy of thinking so intently about one form of heredity that they overlook social heredity. They fall into the grievous error of saying that social intelligence is not worth much when measured by the yardstick which they have devised.

For my own part, I find it to be much more rational to think of heredity as a compound concept and to think of intelligence as a name for a group of human traits. I find my educational creed made clearer when I think of language, and number, and science as a part of the world's intelligence and as forms of personal experience which are acquired after birth, but are no less inherited from earlier generations than are physical traits.

I can make clear what I have been saying by the use of certain illustrations. Foreign language teachers who know the laws of phonetics are able to teach their pupils more exact forms of pronunciation than they themselves possess. In other words, the competent teacher knows something more than pronunciation. He knows the methods of pronunciation. When he teaches his pupils, he directs their development. He produces in them modes of behavior which are more highly perfected than are any instincts which they inherited through the germ plasm. If the hereditarians have a place in their definition of intelligence for those forms of behavior which come through the germ plasm, why do they exclude from their definition of intelligence forms of perfected pronunciation imposed on the organism through the training given by a skilful teacher?

As with the teacher of pronunciation, so is it with every other teacher who is striving to direct the new generation. It is the hope of every thoughtful teacher that he may train his pupils in methods of intellectual adjustment which will carry these pupils further than the older generation has been able to go. The chemist in his research laboratory teaches methods of analysis and synthesis and leaves it for his intellectual descendants to increase the world's intelligence and to add to the world's knowledge with regard to chemical processes through the use of these methods.

There is likewise an endless chain of examples of great historical acquisitions which have transformed the world's methods of intellectual treatment of the environment. The Chinese have a clumsy method of writing. On the other hand, the Occidental world borrowed from some Semitic tribe an alphabet which is infinitely flexible. Was the invention of the alphabet a contribution to the world's intelligence or not? I think the answer is emphatically in the affirmative. The alphabet has done as much to transform the world as has any change known to science as having occurred in the germ plasm of any individual or group of individuals.

As I see the matter, those who think of the I.Q. as an index of that which comes to the individual through physical inheritance are very short sighted; they overlook entirely the broad historical movements which go to explain modern advances in intelligence. Of course, the band of out and out believers in the I.Q. as a fetish to be worshipped and thought of as the sole guide of educational policy must be getting uncomfortably small now that the Stanford group recognizes the fact that significant changes take place in the I.Q. as a result of the action of superior environment. It is my hope that the Stanford group may be persuaded to continue their researches and I have a strong impression that if they continue, they will find grounds for extending the definition of intelligence to include some of the higher powers of thought as conditioned by language, and number, and all that comes after one begins to use his eyes and ears.

There are a number of other matters which attract the attention of the careful reader of the *Yearbook.* I shall call attention to two such because they seem to reinforce very powerfully the position which I have been defending. Chapter XVII by Misses Hoefer and Hardy seems to me to be a very impressive chapter. Briefly put, we may summarize that chapter by saying that pupils who are in good physical condition are likely, on the whole, to show I.Q.'s superior to those shown by pupils who are less favored physically. The advantages of good health do not seem to appear suddenly. Good or bad tonsils are not closely paralleled by high and low I.Q.'s and broad shoulders and satisfactory physical weight are not necessarily connected, so far as the measurements show, with the high levels of intelligence.

The meaning of these facts seems to me clear. Good health is favorable to high I.Q.'s because in general if the physical mechanism is impaired, the nervous system functions deficiently and if the physical mechanism is working normally, all the individual's functions will show favorable effects. On the other hand one unfavorable physical symptom is so insignificant as contrasted with the immense importance of the school arts that there appears to be no relation between intelligence and any particular physical trait. Tonsils, to be quite explicit, are important but they are totally eclipsed by lessons in arithmetic. Broad shoulders are desirable but if one can read, he will be able to get on very well with shoulders that are not up to par. Even people who are underweight may take courage from the measurements of Miss Hoefer and Miss Hardy. They may some day be known as competent citizens if they will only put their faith in the English language and the Arabic numerals.

I would not have any one misunderstand what I am saying. I believe in tonsils and in proper adjustments of weight. I believe in shoulders and lungs. I believe equally in literature and science. My definition of intelligence is based on the observation that the world has made progress in accelerated degree since the time when literature and science came to be recognized as social inheritances of prime importance to the world. I would not commit the fallacy of putting into figures values for which I have no exact measures. If any one were to force me, however, to decide how many points in my I.Q. are directly traceable to the Arabs who, so far as I know, are not in the direct line of my physical ancestry but who gave to my European ancestors a number system, I should be compelled to be liberal with these same Arabs, far more liberal than ten points.

A final contribution to my social inheritance drawn from the *Yearbook* was derived from a study of the chapter by Professor Gates.

This chapter seemed to me to be impressive first of all because it is a report of an experimental study. When science encounters a highly complicated situation, it always begins its attack on this situation by making an analysis and by isolating in this way the factors with which it has to deal. Professor Gates has made an elaborate analysis of his problem. In so doing, he has set a helpful example for the science of education. Careful laboratory experiments are much more arduous than is the wholesale giving of tests. Because tests are relatively easy to administer, we have a flood of superficial studies which have employed tests without analysis and have reached conclusions which are painfully trivial. Professor Gates has used tests but his work is that of a laboratory scientist.

He experimented with two simple functions, namely, tapping and immediate memory. He found that the effects of training were in the first case, that is, in improving the rate of tapping, very slight. He found in the second case, that is, in immediate memory for numerals, that training produces no permanent effects. He concludes from these experiments that the effects of training are in general due to the cultivation of certain techniques or certain tricks of efficiency rather than to changes in fundamental capacities.

I do not know how far Professor Gates will be willing to accept what I have called ''social intelligence'' among his tricks or techniques of adaptation, but to my mind the implications of his experiments are clear. Wherever a pupil finds it possible to adopt a new method of behavior, he has introduced into his life a real factor. If the method of behavior is suited to the situation, the I.Q. goes up. If the method is unsuited to the situation, the I.Q. goes down. Method of response or method of adaptation is a highly important matter and it is this method of adaptation which education aims to guide.

I could wish that Professor Gates had given more attention to some of the higher forms of learning where method of reaction is more significant. There are in the literature some very good examples of investigation which he has overlooked. Sooner or later the list of studies of higher forms of learning will, I dare say, be supplemented by his own investigations. The higher forms of learning are impressively dependent on the acquisition of techniques of reaction. A study of the effects of training in the methods of science, for example, will prove very revealing to Professor Gates when he reaches the point where he is interested in the relative value of theory and practice and he will find ample verification of the view that it is the techniques of procedure which are the real ends of education. I think he will discover also, as has been pointed out in some of the reports of earlier experiments which he overlooked, that these methods of intellectual procedure can be generalized and can be formulated into what we call laws in science.

What I have been saying may be summarized as follows. Throughout this *Yearbook* there is a large encouragement for those of us who believe that education is capable of improving individuals. A favorable intellectual environment does something to pupils. We must find by careful experimentation how to produce and apply favorable environments. We must find what methods of adaptation the race has discovered and we must add these to a pupil's physical equipment. This *Yearbook* ought to put an end to the superficial fatalism that has been rampant in certain quarters and ought to turn educational science into an experimental endeavor to discover how to transmit social inheritances as readily and as completely as possible to all pupils. When we thus understand the function of education, we may confidently define the ends of education by saying that individuals are what they are because of what they inherit from their germ plasms and from their social surroundings. We may think of their function in the chain of generations to be first the acquisition of the best known methods of reaction and second the initiation of new and better devices of adaptation to be transmitted through education to the next generation. The word "intelligence" will then include many different kinds of acquisitions and many different forms of initiative and the I.Q. will be a tool of education rather than a deadening and inhibiting influence on school organization.

READING SEVEN[1]

EXPERIMENTAL STUDIES OF ADAPTIVE BEHAVIOR IN CATS[2]

Editor's Note

Nature and Importance of the Gestalt Movement.—The point of view illustrated in these readings may be traced back to a vague and general beginning early in the history of psychology, but its importance was little understood until recent times. Indeed, its wide applicability to problems of behavior, and the extent to which it might overcome numerous difficulties in current psychology became evident only a few years ago, largely through the work of three German psychologists, Professors Max Wertheimer, Wolfgang Köhler, and Kurt Koffka, whose influence rapidly developed into the proportions of a movement known as the *Gestalt* movement. It was this movement that led to the interest in organismic principles in psychology, now developing in this country, where the term configuration is often used as the equivalent of *Gestalt.*

Gestalt is the German word for *form* or *shape.* In a strict, psychological sense it refers to an experience as an organic whole, one that is subject to analysis into parts but not *composed* of parts. The essential features of a *Gestalt* are (1) its dynamic unity (structured both in space and time), (2) the fact that its various aspects derive their properties from the whole, (3) that it exhibits a quality—a form—corresponding to its own unique organization, and (4) that its details can vary so long as the *form* remains constant. In short it obeys the organismic laws presented in Reading One. To illustrate, a melody is a *Gestalt.* It is more than the sum of individual tones, for the tones can all be present in haphazard order and there will be no melody. It can be played in any key, high on the scale or low, and by any kind of instrument, yet the melody remains. Each tone has its dynamic relation to the melody as a whole—its place in the psychological balance of tones—and derives its musical properties from the whole. It is not a melody until the pattern is *complete.* It commences in a certain way and demands a certain completion. The dynamics of this situation are those already described under the law of configuration. The experience is not that of a melody unless all the conditions are present to control the process from its beginning to its end. Of course, individual tones, taken by themselves as segregated wholes, possess properties in their own right, but these properties are to be considered, later, in this series of Readings.

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[1] Parallels Chapter V of *The Science of Psychology.*

[2] By Donald Keith Adams, Wesleyan University. From *Comparative Psychology Monographs*, Vol. 6, May, 1929. Baltimore: The Johns Hopkins Press, 2-4, 26-28, 33, 34, 35-39, 49-50, 62-64, 91-94, 109-118, 154-162. By permission.

Gestalt came to be applied, as a descriptive term, to all types of mental processes and to co-ordinated movements of all kinds. Professor Köhler made extensive use of the *Gestalt* principles in experiments on apes. He also worked with chickens. These experiments stimulated many others on animals at various levels of the evolutionary scale, and as a consequence of accumulated evidence, comparative psychology, today, is undergoing a radical change.

For a considerable period of years efforts had been made to explain not only animal but human behavior on the basis of an entirely different assumption, namely, that *parts are primary*. This assumption, known in general as atomism, prevailed under various forms. If the problem was that of accounting for mental processes, mental elements, defined as sensations, images and feelings were considered the primary materials out of which all mental life was built through the processes of addition and synthesis. Laws were postulated to account for the fact that these mental elements, by definition unrelated and unorganized in the beginning, ultimately operated within organized wholes. These were called laws of association. If the problem was that of accounting for the co-ordination of bodily movements—walking, manipulating objects, performing all kinds of skilled activities as in typing or playing a musical instrument—response-elements, defined as reflexes, were considered the primary units. Again, laws were postulated to account for the fact that these motor elements, by definition unrelated and unorganized in the beginning, ultimately operated within organized wholes. These laws, called the laws of conditioned reflexes, are still popular as explanatory principles.

The change, therefore, occurring at present in comparative psychology, and in general psychology for that matter, is a change in fundamental assumptions. The mechanistic and atomistic theories offered no place, either in the behavior of the animal or of the human being, for such phenomena as insight, will, and the self. These phenomena were were explained away as unscientific and were studied under the influence of preconceptions that prevented the investigator from seeing them in their true light. One movement in this country—not without its major contributions to the development of psychology—went so far as to rule out any form of *conscious* behavior as legitimate subject matter for psychology. Reflexes were the only materials suitable for the psychologist to study, in a last analysis. This movement was known as behaviorism.

Dr. Donald K. Adams' experiments, here presented in part, were planned and executed with the view of testing the organismic *versus* the mechanistic set of assumptions with regard to the behavior of cats. Do these animals mechanically extricate themselves from a problem situation by sheer repetition of accidentally successful movements? or do they discover relationships in accordance with a degree of insight obviously below the level of man but manifestly above the level of blind trial and error?

If a cat's response is insightful, at the cat's own level, it should reveal the following characteristics of a *Gestalt* or configuration: (1) Evidence of a tension to be resolved; (2) organization with respect to a goal; (3) response to one detail of the total situation in its relationship to other details; (4) sudden learning, that is, the making of discoveries; (5) transposability from one situation to another.

It is impossible, of course, for a human being to see a problem situation as a cat would see it. If the animal appears terribly stupid it is probably not because, in terms of cat-intelligence, it is feebleminded, but because the problem is too difficult. The human observer is likely to apply human standards of efficiency to the cat—an unwarranted bias. Hence, the experimenter must be careful not to deny the cat goals of its own that may not coincide with the goals prescribed by the experiment. Behavior, quite sensible in terms of "cat-standards," might be quite ridiculous in terms of human standards. This means that to evaluate an animal's performance as a trial and error one is to impose a dangerous interpretation upon it. Organismic laws permit of no such interpretation, for in terms of these laws there are no trial and error performances in nature; there are no wasted movements; every movement that takes place occurs in the line of least action, it is a completely organized performance relative to the existing conditions. Does the behavior of Dr. Adams' cats substantiate or fail to substantiate these laws?

INTRODUCTION

The animals to which the methods of comparative psychology have been applied with the most striking success are the primates and the rodents. Of late years the work of Yerkes, Köhler and Bingham has focused attention especially upon the anthropoids. The rat we have always with us. It is instructive to note a characteristic difference in the techniques and procedures used with two such animals, as, say, the rat and the chimpanzee, a difference apparently conditioned in part by such extra-psychological considerations as scarcity and size. Thus we see that most of the rat researches have used animals in more or less considerable numbers, and in general a rather mechanized technique, notably the maze. Researches upon anthropoids on the other hand, have been, for the most part, highly individual, and have used a much greater variety of problem situations. These have consequently entailed rather more elaborate descriptions of actual behavior than merely noting time and number of errors. There is no obvious reason, for example, why rats should not be set problems essentially similar to those customarily used with chimpanzees. Differences in motor equipment can usually be got around by proper adaptation of the situations. Nor is there any purely psychological reason why apes should not be studied by the more or less mechanical methods customarily used with rats.

Whatever may be the reasons for these differences in technique, we have in the cat an organism of a degree of complexity between that of the rodents and that of the primates, which would seem to offer a peculiarly strategic opportunity for the application both of the mass production methods commonly employed with rodents and of the individual methods generally used with primates. The paucity of experimental work upon the cat is regrettable, and at first perhaps surprising, especially in view of the commonly asserted fact that the experimental method in comparative psychology was first applied to cats. Indeed such was the influence of Thorndike's pioneer monograph that Cole was quite justified in saying as late as 1915 that "The psychology of mammals must now cease to be a mere generalization of the psychology of cats." That the psychology of mammals was just this was due almost entirely and solely to Thorndike's experiments of 1897-1898, for very little had been added to our experimental knowledge of cats between that time and 1915.

The original aim of the present studies was to repeat some of Thorndike's puzzle box experiments duplicating his conditions as nearly as possible for the purposes of (1) ascertaining whether or not the behavior of his cats in those situations was characteristic of cats in general in similar situations, (2) of determining the utility of the puzzle box method, or the purposes for which and conditions under which it is useful and valid, if any. Inasmuch as the behavior of my cats in the puzzle box experiments was not always like that described by Thorndike as characteristic and general, I have felt obliged to describe the actual behavior in some detail and at some length. A literal, objective, and consecutive description of behavior is bound to display objectionable composition. "Then," "after," and "later" will appear with wearisome repetition, but they have seemed to be inevitable, and I can only ask the reader's indulgence and perseverance. The remainder of the paper will be found, I hope, more readable.

As the work progressed other questions asserted themselves and methods and procedures were appropriated or devised to answer them. These will be reported in the order in which they were applied to the cats. There is always, of course, in an experimental study of animal psychology the general purpose of determining, in so far as our knowledge of other animals permits, the phylogenetic status of the animal under consideration with respect to some particular or some general functions which may be either behavioral or mental or both; as for example, color or brightness discrimination. In the present studies the sorts of behavior in which we were primarily interested were, in general, relatively complicated adaptations to novel and complicated situations.

In this monograph there will be reported some studies of the behavior of cats by a variety of experimental methods, some of which approximate those hitherto used mainly or solely with primates. The colony of which these cats were members was maintained in the Primate Laboratory of the Institute of Psychology at Yale for three years and a portion of it is continued at present at Wesleyan University. The advantages of a permanent colony are obvious, and in making an inference from the behavior to the mental processes of an animal it is highly essential to know as much as possible of that animal's previous experience. In the case of the animals born in the laboratory this was known at least in a negative way; that is, it was known that they had *not* had experience of situations or parts of situations approximating those to be used in the experiments.

I. Puzzle Box Experiments

Method and Procedure

The experiments reported in the immediately following sections were made with three puzzle boxes of the Thorndike type. These were, in fact, attempts at duplication of three of his boxes, A, B1, and C. They will be described in detail. More complicated boxes were not used because, for reasons that will appear, they rather obscure than clarify the problems involved. One of the incidental products of the present work will be an evaluation of the puzzle box method, and for this reason general discussion of the method will appear in a later section.

The physical conditions of Thorndike's experiments are not clear from his report, nor is his procedure. So far as they were clear, they were followed in the present study, except that the regular food was substituted for the fish that he used as reward. While at first the animals were fed twice a day as described, animals that were being used in the experiments were always fed only once on such days as they were used, at the close of the day's work, whether at six o'clock or at midnight. Unless otherwise noted the following conditions may always be understood:

1. An animal when first put into an experiment had had no food for at least fourteen hours.
2. If an animal failed to escape in half an hour, it was considered to have failed the trial, and was allowed the freedom of the living quarters for at least half an hour before the next trial was made.
3. If an animal failed in five consecutive half hour trials it was considered to have failed the problem.
4. If an animal succeeded in escaping from a box, the next trial was made as soon as it had eaten the reward, usually an interval of two or three minutes. If the interval was longer, its duration will be noted.

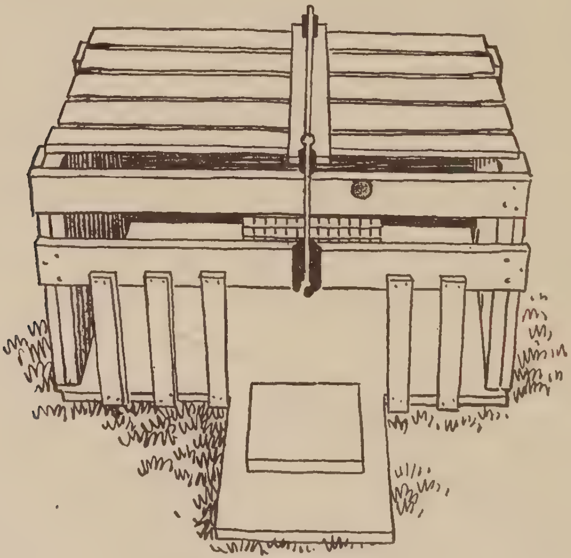

FIGURE 1 (Original FIG. 5)
Box B1

5. The reward, unless otherwise noted, always consisted of one and one half grams (dry) of the regular food, moistened to a paste.
6. The reward was in a flat tin pan, about nine inches in front of the box and a little to the right of its center.
7. The experimenter was outside of the experiment room.
8. The animal was in good condition and of normal weight.

The front of Box A was constructed as follows: a horizontal strip two inches wide was flush with the top. One and three-fourths inches below this was a horizontal strip two and one-fourth inches wide. To this latter were fastened on the outside, five vertical strips, three to the left and two to the right of the door. They were fastened at the bottom to the floor of the box, which projected half an inch forward of the sides. The strips were one and one-eighth inches wide and, except for the ones next to the door, were equally spaced. These latter were just one inch from the edge of the door. The latch was an ordinary barrel bolt with the middle sheathe cut away so that the bolt would not catch. The door was five and three-fourths inches high, and five and three-eighths inches wide, and hinged at the bottom to the floor. The latch was fastened to the second horizontal strip directly above the center of the door. At first the door was pulled open when the bolt was slid by an elastic stretched between the upper end of the door and the threshold, about eight inches in front of the door. This was early replaced by a spring under the door. The top of the box consisted of five two-inch strips running lengthwise of the box, with five one-inch spaces between them, the last strip being flush with the back of the box. The whole box, except for the ends, was made of half-inch pine, as were the others.

Box B1. For this problem I used the same box as for the preceding one, adding a two-inch strip which ran across the box from front to back, and carried a pulley at the back. The string went over both pulleys and down through a hole in the added strip and the last strip of the top, ending at the loop which was hung close to the top, so that the string itself could not be pulled. In order to prevent opening the door by reaching through the horizontal aperture in the front and pulling up the knob of the latch or by pulling in the string where it crossed the aperture, I was compelled to insert a six and one-half inch piece of wire mesh in this aperture, directly above the door. This box is shown in Figure 1.

Box C. A door of the usual position and size was kept closed by a wooden button 3½ inches long, ⅞ inch wide, ½ inch thick. This turned on a nail driven into the box ½ inch above the middle of the top edge of the door. The door would fall inward as soon as the button was turned from its vertical to a horizontal position.

EXPERIMENTS WITH BOX A

Cat number 1, Ace, female, six months old, good condition; had been in the laboratory from the age of three and one-half months.

Trial 1. Ace spent about twenty-eight minutes sitting in the center looking around, and about two minutes in activity, about twenty seconds of which was excited activity occurring nineteen minutes after the cat was put in. At the end of half an hour I entered and opened the door by depressing the loop with a pencil. She was allowed to eat about ten grams of the food and was then replaced in the box.

Trial 2. After one or two exploratory gestures she sat quietly in the center for about five minutes, when we were both startled by a loud commotion among the apes upstairs. She mewed and reached through the front to the pan, which I had inadvertently left just within reach, upset it, drew it in, and ate some of the food, pulling the food—not the pan—through the bars. I entered and replaced it. She immediately tried to reach it again, but quickly desisted and sat quietly in the center for five more minutes. She then pawed the pulley casually, and in so doing caught the string with a claw as it went over the pulley, and in withdrawing the paw, opened the door. After looking around for a minute, she came out and was allowed to eat ten grams. Time, thirteen minutes. After eating—about two minutes—she was replaced.

Trial 3. She first pulled in the elastic which was then used to open the door. It came off its hook when she released it and I replaced it. Her next movement was pulling the string just below the pulley, which opened the door. This time she came out at once and ate the reward. Time, two minutes.

Trial 4. Just after being put into the box, she pawed the latch by thrusting her paw out through the opening over the door, and in so doing knocked the loop with her head, but paid no attention to it. Twice she pulled in and bit the elastic, but for the remainder of the half hour period sat quietly in the center or a back corner of the box. At the end of that time she was released and removed but not fed. There was not more than two minutes of activity in this trial.

Trial 5. Eight hours later Ace was given her fifth trial in this box. This was permitted to continue for fifty minutes in order to see whether or not the dissatisfaction of confinement might be cumulative and eventuate in greater activity. However, only about five minutes of activity occurred—at scattered intervals—in this whole period. Twice in these active intervals she broke the elastic, and I had to replace it. Once she pawed the pulley, but to no effect. *At no time did she scramble and claw indiscriminately.* Her attacks upon the elastic were energetic, but always narrowly directed upon it alone. After fifty minutes, nearly all of which were spent in sitting quietly near the center of the box, I released the latch where she could not see it, allowed her to leave through the door and to eat about five grams of the food, and replaced her in the box.

Trial 6. Immediately upon being put into the box, she pawed once at the elastic and then sat in the center of the box. After sitting there almost motionless for one hour and seven minutes, she pawed the pulley, the door opened and she came out and was allowed to eat all she wanted—about eighty grams—of the food. There were only two acts in this whole period.

Trial 7. Three days later Ace had her seventh trial, which lasted for one hour and ended in failure. Twice during this period she pawed at the latch on the outside. During the first forty minutes, she worked about half the time, but except for the two movements mentioned, all her activity was narrowly directed upon the elastic, which she broke five times. She seemed to try to do this. Her technique consisted usually in drawing in the elastic with a paw, holding it to the floor, and biting and pulling it with her teeth. After forty minutes of this, she stopped working and sat in the center for the remaining twenty minutes. At the end of an hour she was taken out, but not fed.

Trial 8. Ninety minutes later Ace was put into the box for her eighth trial. In the meantime I had taken off the elastic and substituted a piece of steel clock spring (under the door where she could not see it) as the means for opening the door. She sat in the center for four minutes, then pawed several times where the elastic used to be, stopped abruptly, and bit the string just above the loop. An instant later she pulled the string in this way, came out and ate about twenty grams of the food. Total time, five minutes; working time, one minute.

Trial 9. Four minutes later she was replaced in the box, and immediately sat down. After a minute she pawed where the elastic had been, then toward the food pan, and sat down in the center. Five minutes later pawed over the top of the door at the latch and after a few efforts raised the latch by pulling up the knob. She came out at once, but instead of going directly to the food she paused and looked over the latch and associated mechanism on the outside for thirty seconds. Time, seven minutes; working time, one minute.

Trial 10. Two minutes later Ace was again put into the box and again sat in the center. After one minute she pawed tentatively toward the pulley but did not touch it, sat down front center, then pawed where the elastic had been and at the latch. After three minutes in all, she escaped by pulling the string with her teeth.

In the remaining trials there were no extra or ''useless'' movements and the total time varied from six to seventy seconds. The method used varied from time to time, and sometimes she did not set to work at once. These factors, together with the variation in the mechanism, I believe, account for the variation in time required for escape. For the remaining trials, then, I give only the time and the method used. Except for such extraneous activities as washing, sitting down, etc. these are the whole story. Trials ten to twelve followed immediately after trial nine. Trials thirteen to eighteen were made six hours later, and trials nineteen to thirty, twelve hours after these. Except where the intervals between trials are noted it is to be understood that they were about three minutes long.

The following features of the behavior described in the preceding pages should be especially noted.

A very small proportion of the time in the box was spent in activity. In most of the early trials less than one tenth of the time was spent in activity, activity being defined as ''anything but sitting or lying almost motionless.'' In the sixth trial about one third of the time was spent in activity of this broad definition. The average proportion for the first nine trials was about one eighth.

There was only one instance—and that but momentary—of excitement, and only once did the animal mew.

The behavior leading to escape from the box was not stereotyped. I am not insisting that the animal was ''trying'' different methods, but only that the cycles of acts leading to the desirable end were objectively very diverse. The ''methods'' are tabulated in Table 3.

TABLE 3

	Number of Times
By pulling string with teeth	8
By pulling string with paw	4
By pulling loop with paw	4
By pulling pulley, or string on pulley, with paw	2
By pulling loop, holding it between chin and paw, and moving back	2
By pulling string with teeth and paws	1
By pulling loop with teeth	1
By pulling loop with teeth and paw	1
By pulling string with teeth, loop with paw	1
By pulling string to mouth with paw, then pulling with teeth	1
By pulling knob of latch, outside, with paw	1
Not noted	1
Failed	4
Total	31

EXPERIMENTS WITH BOX BI

Cat number 1, Ace, one week after her last trial in Box A.

Trial 1. In this trial the wire mesh shown by the figure to be in the horizontal opening above the door had not yet been placed there, and her first escape was made in three minutes, fifteen seconds by pulling the string outside the box and just above the bolt.

Trial 2. After inserting the wire mesh she was given her second trial, about six minutes later. She first tried for a minute to operate the mechanism as in her previous success. This failing, she pawed three times toward the food, then sat quietly in the center for a minute, then arose and, for the first time, looked around the interior of the box. Theretofore, all her attention had been concentrated upon the front. As soon as her head was in line with the loop, she went directly to it, pulled it, and escaped. Time, 3 minutes 30 seconds.

In every one of the remaining 23 trials, this cat went directly to the loop and pulled it at once. There were no other movements, except in the seventh trial, when she first tried to reach the food directly through the bars. Transfer of the adaptive response to the loop in the new situation was immediate and permanent.

Cat number 2, Deuce, ten days after her last failure in Box A.

Trial 1. Her only activities consisted in washing and once sniffing at the loop. Removed at the end of half an hour.

Trial 2. Three hours later. Her only activity consisted in pulling the loop which she did after sitting quietly in the center for 2 minutes 40 seconds. Time, 3 minutes 10 seconds.

Trial 3. Three minutes later. For 50 seconds she looked about the box, then pulled the loop with her paw and—without letting go of the loop—turned her head through 180 degrees and looked directly at the door. She had not pulled hard enough, and the door was still closed. She resumed pulling the loop, but after several more unsuccessful efforts, reached through the top, around the two inch piece that concealed the string, and opened the door by pulling the string there. Time, 1 minute 40 seconds.

Trial 4. Three minutes later. She went almost directly to the loop and pulled it repeatedly, but not hard enough to open the door. She then pulled the string several times by reaching through the top as in the preceding trial, but did not succeed in opening the door. She lay quietly in the center or washed for the remaining 26 minutes of the half hour period, when she was removed.

Deuce was given four more half hour trials at intervals of three hours, in all of which she failed, but in two of which she first pulled the loop unsuccessfully. She had evidently resolved the situation successfully in another way, namely by adapting to it, for she was frequently heard to purr in these later trials.

Cat number 3, Trey.

1. This cat failed in Box A, only once touching the loop, and then paying no attention to it.
2. She failed in Box B1 in five standard trials during which only one act was directed upon any feature of the box, and none upon the loop.
3. After one demonstration there was no apparent change in her behavior; that is, the ensuing half hour was spent like the preceding ones in lying in the box with no attention to any part of it.
4. After the second demonstration, she performed the required act repeatedly and with vigor, but owing to the imperfection of the apparatus did not escape.
5. After the third demonstration she succeeded in opening the door by pulling the loop.
6. Thereafter she *always* pulled the loop *immediately* when put into the box. There was no elimination of useless movements because there were no useless movements to eliminate, gradually or otherwise. No activity was directed upon any-

thing in the situation except the loop at any time after the first of her five standard trials.

EXPERIMENT WITH BOX C

NOTE:

Box C resembled Box B1 in its general features but the slats (five in number) on the top extended from right to left instead of from front to back. On the front, there were three horizontal boards, the bottom a wide one so that there were two narrow spaces toward the upper part of the box. Ed.

Cat number 6, Fitz, about three weeks after his work in Box B1, was used in a series of experiments with Box C, under standard conditions and with standard procedure. He was much tamer than in the preceding work. In fact he was as tame as any of the cats. This dated entirely from January 19, the date of his work in Box A.

When put in for his first trial, Fitz walked around the box mewing for two and a half minutes, then "nosed" at the back center top, where the loop had hung in Box B1. He continued to walk around and examine all parts of the interior of the box, and after five minutes came upon the button, which he felt tentatively with a paw, the first object he did paw. Two minutes later he pawed tentatively (*i.e.,* in an exploratory sort of way, as if in tactual examination to supplement his visual survey) at top 3 (the top openings are numbered from observer's left to right as he faces front of box), and at top 6. After eight minutes twenty-five seconds in all, he turned the button *almost* to the horizontal, quite abruptly, and with what movements I could not see. In some of the later trials I stayed in the room to observe his technique, and his operations then will be described. But when I was outside the room I could see only a part of the performance, owing to the solid front of the box. At any rate, he did not at this time turn the button quite far enough, and a minute later turned it back almost to its original vertical position. A minute after this he tried to jam his head through top 6, and pawed through top 3 and top 4. After eleven and a half minutes in all, he jammed his head through top 3, and had difficulty in pulling it in. He then tried to jam through top 6 and through top 1, failing in both cases. Fourteen minutes after the beginning of the trial he succeeded in jamming his head through top 2, and thirty seconds of struggling brought out his body.

Trial 2. Before putting in Fitz for his second trial I narrowed the openings of the top by tacking quarter inch strips to the edges of the bars. When put in, he walked around and mewed for one minute forty seconds. Then he attacked the top openings with head and paws, number two—where he had escaped the previous time—first and most. This continued for nearly eight minutes, interspersed with brief attempts at the button, and horizontal front opening 1 (numbered from top down). Then he sat in the left back corner of the box, mewing softly but continuously, for more than six minutes. This was followed by another attempt at the top openings in fairly systematic order with no repetitions, another minute seated in the back, and another attempt at top 2, top 3 and top 4. For the last of these he hung with all four feet from the top and tried to jam his head through in that position. I think this an enlightening example of adaptation, and will refer to it again. Finally he gave over his attempts at the top and turned to the front, pawing once through front 1. Then, with his left paw in that opening, he reached down with his right, took hold of the lower end of the button and pulled it up to his right to the horizontal position and opened the door. Time, twenty-two minutes.

Trial 3. Fitz *immediately* went to work on the button, and escaped in one minute six seconds by pawing it around to a horizontal position, this time from below and to the left.

Trial 4. Escaped in fifteen seconds, turning button to left from below.

Trial 5. Escaped in twelve seconds. The spring under the door was just strong enough to carry the door to the point where it would continue to fall of its own weight if unimpeded. In this instance and several later ones, the cat was close enough to the door to stop it after the impulse of the spring was spent, that is when the door was open about an inch at the top. In all such cases, including the present one, the animal thrust a paw through at the top and pulled the door clear down to the floor.

Trial 6. Escaped in eight seconds, operation invisible.

Trial 7. Escaped in three seconds, operation invisible.

In the remaining two trials, I remained in the room to see how it was done. I stood in back of the box, and it is noteworthy that in these two trials the time was longer than in any of the preceding four; further evidence, I think, that his motivation was social.

Trial 8. He stood a little to his right of the button, put his right paw on the upper (shorter) end of the button, and pulled down and to the right. Simultaneously, he put his left paw on the lower (longer) end of the button and pushed up and to the left. The button could not have been turned with greater precision and economy of effort by any animal. From what I could see of his position and movements in earlier trials, I do not think this method of turning was used consistently. It certainly was not used in the following trial.

Trial 9. He moved the long end of the button through about forty-five degrees to his left with his right paw, then put his nose under it (the long end) and raised it to the horizontal position. His head impeded the falling of the door, and he thrust his right paw over the top and opened the door completely. It should be noted that there was no hiatus between the first movement of the button with his paw and the second movement of it with his nose. The second flowed directly out of the first, *as if he had intended from the start to do just that.* This animal was afterward used as a demonstrator in a number of imitation experiments and his method of operating the button varied greatly, but never was there any cessation of effort, or break in its continuity, until the door was opened to the full. And never after that one time in his first trial, before his first escape, did he turn the button part way in one direction and then turn it back.

II. Generalized Description of Puzzle Box Behavior

1. There are large individual differences in kind and amount of activity in the puzzle box situation. These may be conditioned by size, age, sex, innumerable physiological conditions, the unanalyzed complex of native and acquired characters customarily lumped and called temperament in humans, or by a number of other things. Thus, some cats tried to jam their heads through the openings, but none of the larger cats, and none with large heads ever tried to do that. Incidentally, these efforts to jam the head through an opening are not quite pointless and random: one cat, it will be remembered, escaped from the box in that way, and in most cases the openings were almost wide enough to permit of that. With the experience the animals have had when first put into a puzzle box, that is, from the human point of view, not only the most obvious but eminently the most sensible thing to do.

2. Usually there was little time spent in activity as compared with the amount of time spent in looking over the situation.

2*a*. Activity diminished with failure.

3. In the event of failure substitute activities were often observed. Thus, in the puzzle boxes, as in other problems, notably multiple choice problems, cats were often observed to wash much more persistently and assiduously than they were ever observed to do in freedom. It was noteworthy also, that this usually occurred only when the animals were unsuccessful. Under identical conditions, so far as subject to the experimenter's control, the same animals that washed persistently before success, lost all interest in that activity after the trick had been done. One is inescapably reminded of the child who bites his finger nails when posed a question he cannot answer, or of head scratching in the adult. I have observed a seven-month-old child, who usually sucks his thumb only when very tired or sleepy, to do so almost invariably when he fails in some effort.

4. The first solution was usually accidental in the sense of occurring in the course of activity not specifically directed to that end; though one large animal never did anything but the successful act. Of course, the boxes are so designed as to make impossible any other sort of solution.

5. Attention to the successful act seemed to facilitate repetition. The records indicate that the more deliberate and attentive animals acquired the correct act—*i.e.*, reached a high grade of efficiency—in fewer repetitions and more abruptly than the more excited and inattentive ones. This is particularly clear in animals representing the extremes of these two types of behavior.

6. Indications of excitement were not uncommon, but the extreme excitement described by Thorndike and characterized by me as "tantrum" behavior, was relatively rare and was not persisted in. I do not know how to account for this difference. Differences in food, physical environment, health, "temperament," handling, isolation of experimenter, etc. are suggestions, but their relevance cannot be estimated, due to insufficient description of Thorndike's conditions. It is worthy of note that my later group of cats, which were probably in better health, displayed somewhat more excitement than the earlier group, though by comparison with Thorndike's even they were downright apathetic. In any case, as I indicate in criticising Thorndike's technique, an animal in the "fury of excitement" de-

scribed by him as characteristic, is not a fit subject for experiment upon the "nature of the associative process."

7. In general, the learning cannot correctly be described as a "gradual elimination of useless movements." In the occasional instances when the learning did approximate a gradual elimination of useless movements, it was invariably associated with excitement and consequent inattention.

8. In general, the total time consumed was not very relevant, and certainly cannot be taken as a measure of facility of performance or of the "progress of the formation of the association." This will be evident to the reader who has taken the trouble to go through the detailed description.

9. The particular food used as reward had almost no bearing upon the behavior prior to the first success, and probably little bearing thereafter. The first part of this proposition seems to be established by my observations, but the second part cannot be stated with finality.

10. Very diverse modes of operation of the mechanism were used by the same animal in the same puzzle box, at different times. I have called attention to the case of Ace in Box A, in a table at the end of the description of her behavior. As may be seen from the other descriptions this variety of operation of the mechanism was the rule. The variation persisted after the time had reached a low and relatively constant level. This finding seems obviously incompatible with Thorndike's hypothesis of association between the sensory qualities of the situation and a specific impulse to a specific act.

11. Transfer of the correct act from the loop in front to the loop in back was usually immediate and permanent. The case of Ace in Box B1 is the clearest example of this.

12. There was some evidence of learning by "imitation." There is clear evidence (*cf.* Trey in Box B1 and Ace in box C) that attention may be directed to a specific region or even a specific object in an already familiar box by seeing several successful operations of the mechanism by another cat.

13. There is no evidence of learning by being put through an act. However, there is some slight indication that putting through may in rare instances direct attention upon a region or object in the situation. In general it is safe to say that the situation of being handled renders attention to the act through which the animal is being put highly unlikely.

III. String-pulling Experiments

The experiments described in this section were suggested by those of McDougall and McDougall (1927) upon rats, some of which I saw. At about the same time apparently, Drescher and Trendelenburg (1927) were using similar problems upon apes, monkeys, dogs and cats. McDougall's rats readily pulled in a string hanging from their nest when appropriate food was fastened to the lower end. But Drescher and Trendelenburg found only the primates among their subjects able to pull in even a horizontal string, with the attached food in plain sight. The apparatus and procedure in my experiments differed somewhat from those used by these investigators.

A cage with wire panel was in the center of the room. A piece of string was tied to the center of the top, and allowed to hang down to the floor in the center of the cage. It was a little longer than necessary just to reach the floor and consequently not taut. A small wooden skewer was tied to the lower end, and on this were impaled several small cubes (one or two centimeters) of cooked beef liver. The liver was thus about 45 cm. from the walls of the cage at the floor, and at least twice that distance from the top, the only place from which it could be obtained. It did not depend from the string, but lay on the floor. The string was a strip of felt about 6 mm. wide and about 3 mm. thick. In order to get the liver, the animal would have to climb to the top of the cage, go to the center, and pull up the string as best he could with the manual equipment he possessed. There was no special way in which this would have to be done. I was interested in seeing at least whether the cats would paw through the wire netting directly toward the liver, which was there well out of reach, or would inhibit that natural response through visual perception of the distance.

The first of these experiments were made about ten o'clock in the evening, that is, about three hours after the remnants of the animals' daily feeding had been removed, and they were presumably not very hungry.

The first animal put in, Chip, female, number 11, aged nine months, walked all around the cage several times, looking over the whole situation from all sides. However, she did paw through the wall toward the liver several times. Finally, after two minutes and forty seconds of attentive examination, she jumped to the top of the cage. She walked half way around the edge of the top, pawed vaguely through the top at the edge and sat down. After almost two minutes, during which she frequently looked down at the meat but never went to the center, she jumped down and sat in one corner of the room, evidently giving up. After three minutes more, during which she did nothing, I took her out.

The next animal tried was Tom, male, number 10, a brother and litter mate of the preceding, who was being used in the multiple choice work but who had never in the time I had him pulled or played with a string. I obtained him when he was four months old, and while he may have played with strings before that, he certainly could never have been in a situation with all, or anything like all, the features of this one.

Tom walked all around the cage and looked over the whole situation from all sides, but did not once paw through the sides toward the liver. He frequently looked up the string to the place where it was tied, sometimes staring intently at it for some seconds. Finally, after one of these pauses, when he had been in the situation a total of two minutes and thirty seconds, he turned away suddenly from his intent stare, went to the right back corner and climbed to the top of the cage. He went directly to the center, reached through the top and pawed up a loop of the string, but was unable to bring this through the wire. He persisted for about three minutes, sometimes turning his attention to the short loose end beyond the knot. After six minutes in all, I took him out and changed the setting: instead of tying the string to the wire of the top, I tied it to a light stick about 15 cm. long, sufficient to keep it from going through the top, but light and small enough to be moved. The string was hung in this way at the center of the top, and Tom was readmitted.

This time he spent only fifteen seconds in examination from the floor, then went to a corner and climbed up. He went directly to the center, pawed the string where it was knotted around the stick, moving the latter about 5 cm. He then took the string in his teeth, moved backward about 15 cm., and sat down. Then with a sweeping motion of his right foreleg, using it as an arm, he took hold of the string with his paw as far in front of his mouth as he could reach, swung the paw as far behind as he could reach, raising the liver perhaps 30 cm., stood on the portion of string gained thereby, and took a new and nearer hold with his teeth. Immediately, then, he turned his back on the reward, and jumped down from the cage, keeping the string in his teeth, and with the—to me—quite evident and confident anticipation that the liver would follow. This brusque jerk brought the liver up to the wire top so smartly that most—I thought at the time, all—of it was knocked off, and fell inside the cage. The skewer caught in the mesh where it should have come through and did not follow Tom down. It was this catch that took the string out of his mouth as he landed from his jump. Thus accidentally deprived of his reward, Tom gazed at the pieces of liver inside the cage, and for more than half a minute did nothing but that, so I entered and gave him another piece, which he ate. It is unfortunate that the liver was not fastened more securely than by a skewer but I did not anticipate a solution by jumping down. I expected, at best, to see him attempt to haul it up hand over hand. The whole thing happened so quickly that I am fortunate in having the observations of details that I was able to get.

The next day, about 21 hours later, he was introduced into the same situation, except that the liver, instead of being fastened with a skewer, was *threaded* on a bit of cord which was tied to the suspended string. He looked up the string from the back of the cage, and from the right, then, about five seconds after entering, climbed the right-back corner, went directly to the center of the top, pawed the stick about 5 cm., took the stick in his mouth, turned, ran to the edge and jumped down. This time the liver followed, and he ate it. The total time was not more than ten or fifteen seconds.

Twenty minutes later, I put him into the original situation, *i.e.*, with the string *tied* to the wire top at the center. I wished to see whether he would differentiate behaviorally between this and the situation he had mastered, and if so, whether he would succeed in pulling up the liver hand over hand, or would persevere in attempting to do that. He climbed to the top immediately he was brought in, went directly to the center, and repeatedly pulled through a loop of the string. He persisted for about a minute, then jumped down and looked over the situation attentively from all sides. Twice more he went through this cycle of effort at the string and resurveyed from the floor. Then he gave up momentarily and wandered about the room, but soon returned to the task. After about a minute of pulling through a loop, only to lose it, he took a loop thus gained in his teeth, and tried for more with his paw. Apparently unable to attend to the two tasks at once, he lost both, gave up, jumped down, went to the door of the experiment room and mewed. He was taken out after ten minutes in all. More than half of this time was spent in active effort to pull the string through the mesh. His final effort, when he held a previously gained loop in his mouth, while reaching for another with his paw, seemed to me especially significant. In a human this behavior would inevitably be called ideational, and I can see no escape from the conclusion that it was ideational in the cat.

Nine days later, Tom was given his third trial in the situation where the string was fastened to the small stick instead of being tied to the top. Again the liver was threaded instead of skewered. This time he looked over the situation for perhaps half a minute, then climbed the cage, seized the stick in his teeth without first pawing it, and immediately jumped off the cage. This time also there was an accident, but a more fortunate one than on his first trial. The chunk of liver caught at the top, but did not come off. This left the stick suspended diagonally about 20 cm. below the top near one corner, and against the side of the cage. Tom, again deprived by accident of his reward, looked over the whole situation. Then he stood up against the front of the cage and pawed the stick until it hung about half way down the middle of the front. He pulled the stick hard and repeatedly with teeth and paws for nearly two minutes. This was not continuous, of course. He frequently paused to look over the situation, sometimes staring fixedly at the liver, the stick, or the string. Finally, after one of these pauses when he had stared at the liver for some seconds, he turned sharply, ignoring the stick, went to a corner and climbed up. He went at once to the liver where it was caught at the center of the top, pawed it out, took it in his teeth, and jumped down. He did not eat it at once, but played with it for a moment, then ate it.

A few days later I tried Duchess (female, 18 months old) and Sarah (female, 7 months old) in the same situation. Both climbed the cage, but neither paid particular attention to the string, and each quit after about five minutes and was removed after seven minutes. I then gave Chip a second trial, and she worked at the string for about a minute, but then gave up, and after doing nothing for five minutes was removed.

Further experiments of this sort were deferred until I should have devised some way of preventing the liver from catching on the wire, and they have not yet been done. A wooden or metal cone on the string just above the liver may remove this difficulty. Tom was never again used in this situation and died of hairballs in the summer of 1927.

Horizontal String-pulling

For these experiments the same cage was used as in the preceding experiments, except that the wire front panel was replaced by one of horizontal brass tubes 1 cm. in diameter and 3.3 cm. apart, and the right and left sides were covered with quarter inch mesh wire fabric, so that the cats could not reach through the sides. The strings were pieces of felt weather strip, about 2 cm. wide and 3 mm. thick, and in these experiments were 210 cm. long. To the end of the string there was fastened a small shallow pan, a friction top can lid about 8 cm. in diameter. A piece of raw liver was placed on this, and the string was laid loosely in a right angle. The pan was near one side, and just out of reach—about 30 cm.—from the back. The string ran clear across the cage, always out of reach from the back, then made a right angle, and ran out just through the front. It was not stretched, but laid loosely so that there were waves in it. This provision, and the turn, were used in order that the string would have to be pulled repeatedly or persistently before the pan moved. In general, more of the string had to be pulled in before the pan would move, than would then suffice for bringing the pan outside the cage. A false bottom, painted gray, raised the floor of the cage two inches, flush with the bottom of its frame.

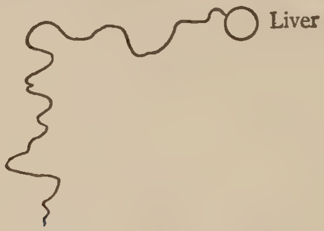

FIGURE 2 (Original FIG. 15)
HORIZONTAL STRING

The following cats were tried in this situation, and behaved as described:

Kim, male, 9 months old, used in lever experiments: mewed and ran around room, paying no attention to liver or problem; excited; removed after five minutes.

Jill, female, 11 months old, used in lever experiments: saw liver, and pawed wire at liver, then paid no further attention to it, but walked around room and mewed; moderately excited; removed after five minutes.

Bill, male, 22 months old, first time in experiment room: sat in front of observation room door, washing himself and mewing; paid no attention to liver or problem; removed after five minutes.

Smoke, female, 9 months old, used in lever experiments: sat quietly at observation room door and did nothing; paid no attention to liver or problem; removed after five minutes.

Duchess, female, 28 months old; used in lever and vertical string experiments: paws string at once, but immediately sees and goes to liver; pays no further attention to problem, but walks around room for two minutes, then lies down on top of cage; removed after five minutes.

Siki, male, 17 months old, previously tried in several experiments, but always became excited by situation of being alone in room, and never worked at any problem previously: mewed and ran around room for five minutes, paying no attention to food or problem. Then I entered. He rubbed around me and purred for about a minute, then set to work. He pawed the wire outside the liver and occasionally pawed the string. After twelve minutes in all, the string was straight, and the next time he pawed it, the liver moved. Immediately he was all attention, strained and tense. He took hold of the string with his paw, stared straight at the liver, pulled, watching the liver move, then looked down at his paw. This procedure was repeated almost identically. Then he pulled again, this time watching his paw while pulling, and looking at the liver afterward. Then he pulled hand over hand until the liver came out, and ate it.

Five minutes later he was given another trial in the same situation. For five minutes he ran around the room and mewed, and at the end of that time I entered. He rubbed around me for about half a minute, then went to the string and pulled out the liver in 25 seconds.

In all subsequent trials, he always pulled the string as soon as put into the room, and always obtained the liver in 25 to 30 seconds. Thereafter, also, he was a useful animal, and worked, although not very persistently, on any problem.

Nip, male, 10 months old, previously used in lever and delayed reaction experiments in same room: looks over situation and sniffs at liver; paws string in a dilatory sort of way, sometimes leaving it to walk around cage, until the string is straight from liver; then pulls string hand over hand, watching liver all the time until he gets it. Time, 2 minutes.

In the next trial, five minutes later, he pulled the string as soon as he was put in and got the liver in 40 seconds. In all subsequent trials he did the same, and his time varied between 30 and 40 seconds.

Buck, male, 8 months old, previously used in lever problems in same room, rather kittenish and playful: walked around cage, pawing wire outside liver, reaching through front and back toward liver, and usually pawing the string when he passed it on his rounds. Finally the string was tight, and the liver moved the next time he pawed the string. As soon as this happened, he rushed to the other side of the front and reached directly toward the liver, which was still out of reach. He did not persist in this, but returned to the string, pulled it until he had the liver. Time, 6 minutes, 10 seconds.

The next trial, five minutes later, was much like the first, except that after the string was straight he persisted at it and pulled it in. Time, 2 minutes, 10 seconds. In all subsequent trials he pulled the string as soon as he was put into the room, and got the liver in 35 to 50 seconds.

Tabs, female, 10 months old, previously used in lever experiments: explored room, and played with string. This was sheer play, and there was no attention to the liver. After 7 minutes the string was nearly straight from the liver through its entire length. Tabs was lying at the end of the string, more than a meter away from the cage, her left side toward the cage. At 8 minutes 15 seconds, she started again to play with the end of the string, still lying down, and in so doing moved the liver. She evidently heard the pan move, for she could not see it. She sat up and looked straight at the liver, motionless and tense, ears up and eyes wide open, for 15 seconds. Then she got up, walked to the cage and looked at the liver from the front and side. Then she left the cage and walked around the room. When she came to the end of the string she again pawed it playfully, and again moved the liver accidentally. This time, however, after looking for only two seconds at the liver, she went to the grill front of the cage and pulled out the liver hand over hand at once. Time, 9 minutes 20 seconds.

In her next trial, five minutes later, Tabs pulled out the string at once and got the liver in 20 seconds. In her subsequent trials she always pulled the string at once, and her time varied between 20 and 25 seconds.

Tar Baby, female, 4 months old, first time in experiment room: much interested in liver; went around cage repeatedly, reaching toward liver from back, left and front; after five minutes she began to pull the string, inattentively until the string became taut and the pan moved, then displaying lively but momentary interest; tried again to reach liver directly from back, left and front for forty seconds, then returned to string and pulled out liver. Time, 6 minutes 15 seconds.

In her second trial, the next day, and in her third and fourth trials, a day later, she pulled the string immediately upon being put into the room, but did not persist until after, through intermittent pulling, the string was straight. Her times on these trials were 4 minutes 50 seconds, 4 minutes, and 2 minutes 40 seconds. In the next two trials, given the same day, and in all subsequent trials, she always pulled out the liver at once, and her time varied between 20 and 35 seconds.

Each of these animals was given not less than ten nor more than fifteen trials in all. The cage was rotated through ninety or one hundred eighty degrees between trials without impeding the animals' solutions. Also, the string was laid sometimes to the right and sometimes to the left.

Sixty days after the last of these trials, each cat was replaced in the same situation. Each recognized it and obtained the liver within the time in which it had obtained it in the later trials of the original series.

It should be noted that no cat that succeeded once ever failed in a subsequent trial; and that most of them attained their minimum time in their second or third trial.

More than a year earlier Chip, a female, then seven months old and not previously used in the experiment room, had learned in one trial of seven minutes to pull out a piece of cooked liver tied to a string. In these experiments the string used was a carpenters' chalk line, which was much harder for the cat to hold than was the weather strip used in the later experiments. Also, the string was laid straight, instead of with a turn, so that the first pull on the string moved the liver. Inasmuch as there was no pan used, the liver moved noiselessly. After her first trial, her time varied between 25 and 30 seconds, and there were no excess movements. Her first three trials were on the same day. A week later she was given her fourth trial, which took 25 seconds.

IV. Summary and Conclusions

The inferences of mental processes and contents that occur in this section have been made by the application to specific instances of behavior of the following principle. *Any experience or mental process in another organism can be inferred from structure, situation, history, and behavior only when a similar experience or mental process is or has been invariably associated with similar structure, situation, history and behavior in oneself; and the probability of the inference will be proportional to the degree of the similarity.* Reference will be made in each case to the pages where the descriptions of the behavior involved occur, and the reader may check the inferences from these.

A series of experiments were performed upon cats in three of the Thorndike puzzle boxes. The behavior is described in detail and generalized. In each case the presence of what Hobhouse calls practical ideas is inferred. The presence of articulate ideas is inferred from the behavior of Pete in Box A, from that of Trey in Box B1, from that of Fitz in Box C and is suggested by other behavior.

The use of articulate ideas is clearly indicated by the behavior of Tom in the vertical string-pulling experiments. The behavior of most of the animals in the horizontal string-pulling problems permits the inference of practical ideas. In most of these cases maximum efficiency appeared in the second or third experience.

To supplement the foregoing summary of my experiments, it seems desirable to give a generalized description of them. Certain features are common to the diverse situations used, and there are characteristics common to the diverse behavior observed. These may be abstracted as follows:

1. There is a cat. If one wished to be esoteric he might prefer to call it a proton-electron aggregate, which is thought by some to be much more scientific. Certainly, according to the physics 1920 the cat *is* a proton-electron aggregate. Whether this will still be "true" in 1930, we do not know. In any case,

it is not my task as a psychologist to study the behavior of protons and electrons, but that of certain of their aggregates which behave as units. The basis of differentiation between sciences seems to be their characteristic units of analysis, and that of psychology is the whole organism. Ultimately, we are told, physics will compass the description of all phenomena. The people who say this usually mean physics as we now know it, or even the classical mechanics. In this meaning there are good mechanistic grounds for doubting the assertion. In any case, much may be learned about animal behavior before that ultimate of knowledge is attained, and this will not be materially impeded by calling the cat a cat.

2. There is something the cat wants. If you prefer, you may say instead, there is a special (described) physico-chemical disequilibrium in the proton-electron aggregate, and perhaps you can describe it. I prefer to say simply that the cat wants the thing, because that is more objective and more accurately descriptive. Objectivity, so far as I can learn, has no empirical meaning except unanimity among observers; and I think more people would see that the cat wants the thing than would see the particular disequilibrium that you see in the proton-electron aggregate. It is more accurately descriptive to say the cat wants the thing, because that will remain ''true'' no matter what we may find out about protons and electrons; whereas better knowledge of such matters may reveal that the hypothetical disequilibrium is quite different from our present notion of it. However, the more periphrastic expression commends itself to some people, and so long as we understand each other there is possibility of agreement as to facts. Incidentally, the thing wanted may be a state of affairs, such as freedom from constraint, and the thing may be wanted very much or very little.

3. There are other things too. Nearly everybody calls these the situation. The disposition of these other things may be such as to facilitate or to impede the cat's getting what it wants.

4. In one way or another, after certain movements with respect to the situation and the thing wanted, the cat gets what it wants. This also can be translated into behavioristic terms: certain changes occur in the disequilibrated proton-electron aggregate, after which there is relative equilibrium. Again the simpler description seems more objective, more completely descriptive, and less speculative. Each one of the general facts in this list can be translated into a form that would be acceptable

to the behaviorists, and in each case this would be found less comprehensive, less exact, less descriptive, and less objective. I shall shirk the task of translating the remainder of the list. It is a job that cross-word puzzle fans might enjoy.

5. There is the cat again.
6. There is something the cat wants.
7. There is another situation, *more or less* like the first. This similarity to the earlier situation may be very great or very slight.
8. The cat gets what it wants more readily than it did before, that is, with greater economy of movement. This economy may be small or great. (Sometimes, of course, it is zero or there are more movements. We are describing the more general case.) If the economy is small, it is often called trial and error learning. It might also be called a small insight. If the economy is very great, it may be called insight, or a big insight, or mental trial and error.
9. Series of events such as the preceding eight occur many times, some under experimental conditions, but most of them not.
10. Sometimes the situation is not *very* like any one earlier situation, but is more or less similar to a number of earlier ones.
11. Under these conditions the cat sometimes gets what it wants very suddenly, and with great economy of movement, after a period of no evident progress.
12. Thereafter in situations *very* similar to this last one the cat uniformly gets what it wants with great economy of movement. These last two phenomena are often called insight behavior.

The foregoing are the general facts of adaptation as I observed them in the cats, stated in terms of objective behavior, with mention of various terms that have been applied to them by other writers. I have indicated earlier the inferences made from these facts to mental processes. What, then, of the psychological theory of adaptation in animals?

The psychological theory of animal learning, or as I should prefer to call it, of adaptation, seems at the present time to be emerging from the morass into which it was led by the *ignis fatuus* of behavioristic claims for the conditioned reflex.

I confess that I have never been able to understand how behaviorism, in the effort to make an universal formula of the conditioned reflex, has succeeded in ignoring the well known characteristics of that phenomenon. I mean the facts that (1) the formation of a conditioned reflex requires very many repetitions, (2) it requires a degree of freedom from extraneous stimuli that can be obtained only under very elaborately controlled conditions, and (3) a conditioned reflex disappears after a very few repetitions without the original stimulus. These seem to me insuperable obstacles to the sweeping application of this important phenomenon to all the problems of behavior which behaviorism attempted.

The behavioristic attempt to describe all behavior in terms of reflexes (simple, chained and conditioned) seems to have been motivated largely by an atomistic bias, possibly inherited from associationism, and by the incorrect impression that this form of description was more mechanistic than psychological description. One fundamental source of this error seems to have been a misconception of a number of our practical, conventional, fictional dichotomies as "real." Among the conventional dichotomies that have at one time or another served a useful purpose but which when uncritically used become vicious impediments to scientific thought are those of subject and object, mind and body, structure and function, innate and acquired, instinctive and learned. This is not the place for an examination of the grotesque progeny of such misconceptions, nor yet for a demonstration of the present limits of utility of the fictions mentioned. It is permissible, however, to suggest a point of view more in harmony with current biological thinking.

The obsolescence in biology of the hard and fast distinction between structure and function is apparent in the continual and automatic evolution of individual morphologists into physiologists. But this advance is also quite explicit, as witness the following from C. M. Child's *Physiological Foundations of Behavior* (1924, p. 12). "The biologist, particularly the zoologist, has in the past been inclined to look at life from the morphological side and to attempt to conceive it in morphological terms, but it becomes increasingly evident that morphology must sooner or later be interpreted in dynamic terms, that is to say, in terms of the behavior of the system in which it appears. That the organism is primarily a behavior pattern in a protoplasm of specific constitution is the principal thesis of this book." How different this is from the enthusiastic atomism of the palmy days of the cell theory, as well as from the morphological slant of biology a generation or two ago.

Concerning another fictional distinction I quote from William

Morton Wheeler's superb *Ants* (1913): "Biologists find it increasingly difficult to draw a hard and fast line between instincts and reflexes, or between either of these and the simple vital activities of protoplasm." An instance that he gives will illustrate the point. One species of ant in passing from the larval to the pupal state develops a pupal skin. Another species spins a cocoon. Is one of these (instinctive) behavior and the other "merely" growth?

I would urge further that the distinction, useful within certain limits, between even learning and the simple vital activities of protoplasm such as growth and development, must be used with extreme caution. Cannot the embryo be regarded as an organism adjusting to a limited environment, the uterus? Cannot the adult individual be regarded as a developing embryo in a larger uterus, the world?

All this is by way of a necropsy upon the behavioristic conception of the organism as a completed structure, like a machine, such that if a certain button be pressed a certain reaction would ensue. This was not mechanism, but mere machine-ism. An important source of this error was the failure to comprehend the unity and integration of the organism, now rapidly becoming commonplaces of biology. It was a failure to see that the behavior of the whole organism is different from that of the sum of its parts, even though those parts (as in the conditioned reflex) remain in the intact organism.

This was, of course, not the only source of the machine-istic error. Another was the vulgar misconception of causation. This is the doctrine that the prior of two invariably associated events has some mysterious and non-mutual relation, besides priority, to the latter. But we need not push the necropsy any farther.

The theory that asserts itself from the facts disclosed by my investigation might be stated at some length but it would be so nearly in agreement with the theory of Tolman (1928) that its statement in my words might unduly emphasize minor differences and I prefer to direct attention to our essential unanimity. I therefore state that in my opinion the experiments reported in this monograph confirm Tolman's theory in detail.

That this is not a mere shirking of labor will be evident, I hope, from the entire treatment. As additional demonstration that our agreement is real, I quote the summary of my dissertation which was deposited in the Graduate School of Yale in May 1927, before the publication of Tolman's article.

"1. In the process of adaptation or adjustment to the experimental environments provided, the cats gave evidence of continual use of tied ideas, common use of explicit ideas, and occasional, but rare, use of free ideas.

"2. In view of the paucity of the cat's associations of ideas of these artificial environments and of the fact that the frequency of free ideas is a function of number of associations, the cats probably use such ideas in their normal environment, of which they have numerous associations, much more commonly than they could be inferred from their behavior in the experiments.

"3. Behavior indicating perception of similarity in difference was observed, and considered to represent an elementary level of inference.

"4. Motor and perceptual adaptation were considered to be indissolubly associated and to develop concomitantly.

"5. Motor adaptation was considered to consist in the development of an act, organized with reference to a goal.

"6. Perceptual adaptation was considered to consist in the development of an object.

"7. Insight was considered to be a special case of adaptation, both perceptually and behaviorally.

"8. The puzzle box technique, as used by Thorndike and myself with the subject inside, has slight utility for the study of adaptive behavior."

This dissertation included only the puzzle box experiments and the vertical string experiments. In the light of my later experiments I should wish to modify these conclusions somewhat, but the modifications would be mainly in the direction of increased agreement with Tolman's theory, his summary of which is here quoted.

"(1) All learning is to be said to arise out of an initial postulation of (insight into) the goal position, and to end in a new improved, or added to, postulation of (insight into) such goal position.

"(2) All learning may be said to involve the representation of the ends of acts at moments before their actual occurrence.

"(3) When these represented ends of acts are situations which when actually present lead at once (given the animal's innate and acquired endowment) to further appropriate responses, then the propensities towards the acts leading to those ends will become strengthened.

"(4) When, on the other hand, these represented ends of acts are situations which when actually present lead only to negative or avoidance responses, then the propensities towards the acts leading to those ends will become weakened.

"(5) The higher the animal, the fewer the number of experiences of an act which are probably necessary to achieve such representations of its end, and the clearer and more accurate such representations themselves probably are.

"(6) The higher the animal, the more it would seem that these representations can be played with and manipulated; the more the animal can mentally add and subtract the acts to produce new representations; the more, in short, he can achieve 'foresight,' as opposed to mere trial and error solutions."

GLOSSARY

Anthropoids, the "highest" apes, most resembling man.

Associationism, movement in psychology which attempted to account for consciousness and behavior by laws of association. Parts were supposed to become associated together into an organized whole by being repeated together, thus accounting for continuity and organization.

Atomistic Bias, bias involving the assumption that wholes can be explained in terms of their parts; mechanistic bias.

Behaviorism, point of view denying the legitimacy of including mental processes in the subject-matter of psychology.

Chain Reflexes, several reflexes joined successively with one functioning as the stimulus or cause of the next.

Comparative Psychology, a comparative study of animal and human behavior.

Conditioned Reflex, a stereotyped unit of muscular movement brought about by a substitute stimulus.

Dichotomy, condition of being divided into two parts of opposite nature; dualism.

Esoteric, (evidently as used) posing as highly sophisticated; saying something designed for the specially initiated; condition of not being able to get beyond itself or change into something else.

Ignis Fatuus, "chasing circles."

Mental Content, mental process; constituent of a mental process.

Necropsy, autopsy.

Objectivity, having the attribute of being external to the observer, and real.

Periphrastic, superfluous use of words.

Phylogenetic, pertaining to racial descent and evolutionary origin.

Primates, the highest order of animals; begins with the monkey; includes the apes and man.

Proton-electron Aggregate, an aggregate of very fine units of electrical energy positively and negatively charged, respectively; "matter."

REFLEX, stereotyped unit of muscular movement.

RODENT, class of animals with teeth especially adapted for gnawing. Examples: rabbit, mouse, squirrel.

SUBJECT AND OBJECT, pertaining to a knower and a known, respectively.

TIED IDEAS, EXPLICIT IDEAS, FREE IDEAS, ideas representing degrees of transferability from one situation to another, tied ideas representing the least and free ideas the most transferability.

READING EIGHT[1]

THE SOLVING OF PROBLEM-SITUATIONS BY PRE-SCHOOL CHILDREN:[2] AN ANALYSIS

Editor's Note

It is of paramount importance for the educator to understand fully the nature of insight in order that mental development may take place in the child with the least amount of wasted time and energy. The majority of investigations on the intelligence of children, in recent years, have been in the field of mental testing. As a procedure, testing has contributed much less to an actual knowledge of the child's mental life than might be supposed from a voluminous literature. This is because testing is a standardized and stereotyped procedure. Before the behavior of a child can be adequately analyzed, it is necessary to make controlled observations under changing conditions. Then, by eliminating certain conditions while others are controlled, it can be ascertained which are essential and which are non-essential to the mode of behavior under investigation. This is the only satisfactory way to obtain a complete knowledge of any process. The procedure is called *functional analysis.*

Köhler adopted this method in his experiments on apes by systematically varying the degree of *difficulty* of the problems, while other conditions remained relatively constant. One of the simplest problems was that of obtaining a banana outside of the cage, but attached to a string; another, somewhat more difficult, consisted of a banana, beyond reach, and a stick that could be used as a tool with which to bring the banana closer. Another consisted of fruit suspended from the top of the cage, and a box off to one side that could be moved. In still another, the fruit was suspended so high that two or three boxes piled on one another were necessary in order to reach the goal.

Dr. Augusta Alpert has recently employed a similar method in studying the behavior of the pre-school child. Certain of her data are included here, together with her comparison of results with those of Köhler. The bearing of these results on problems of education can readily be deduced from her conclusions. For example, it is evident that a random, or trial and error, method of attacking any problem is unnecessary and wasteful. When an animal or a human being adopts such a method it is obvious that the problem is too difficult to fit the subject's level of insight. A new situation should be met successfully the first time. Drill or practice, in the conventional sense of the term, is artificial and wrong. Where learning or mental development of any kind is involved, the problem should be simple enough at the outset to permit an immediate solution; the next time the problem is presented it can be made a little more complex, yet it will be solved immediately without a random procedure. In this fashion the learner *grows* to more and more difficult tasks.

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[1] Parallels Chapter V, *The Science of Psychology.*

[2] By Augusta Alpert, Teachers College, Columbia University. From *Columbia University Contributions to Education, Teachers College Series,* No. 323, 1928, 1-9, 10, 11, 14, 17, 20-24, 25, 26-27, 28-32, 35, 38-41, 50, 53-59, 60-63, 65-69. By permission.

When the tasks given to the learner are so graded in difficulty that each new problem is solved easily and perfectly the first time, the procedure is known as *pacing*. Pacing is based upon a physiological activity that takes place in the nervous system. This process is *maturation*, a growth process upon which the development of insight rests. Not only is maturation the basis of mental development in general, but also of learning anything, such as languages, shorthand, or tennis. The nature of maturation cannot be discussed here, but in its grosser aspects it is a gradual increase in the complexity of brain-structure and a gradual differentiation of energy-configurations within the brain.

INTRODUCTION

Statement of Problem

The "almost human" behavior, to use Yerkes'[3] phrase, of the chimpanzees which served as subjects for Köhler's experiments conducted at Tenerife and reported in his book, *The Mentality of Apes*,[4] suggested itself to the writer as an excellent starting point for a study of the solving behavior, and principally of the solving methods, of the pre-school child when exposed to problem-situations. Such a study constitutes the primary purpose of this investigation. The secondary purpose, a comparison between the solving behavior of children and that of apes, is subsidiary to the first and to that extent is the method or means of approach rather than the purpose or end of the investigation.

Forty-four children attending the Nursery School of the Institute of Child Welfare Research, Teachers College, Columbia University, were used as subjects. They ranged in age from 19 to 49 months.

GENERAL DESCRIPTION OF TEST SITUATIONS

Series I

Situation A. An attractive toy (balloon, duck, dog, doll, bird-on-ring, airplane, or string of beads) is suspended from the ceiling to within about 4 feet of the floor, by means of a red cord, such as is used for tying small parcels. The toy is out of the subject's reach. Five feet away, diagonally to the right of the suspended toy, is a green, hollow block 9 inches by 12 inches by 15 inches. This was taken from the roof equipment of the Nursery School attended by the subjects and was therefore a familiar object. The solution of the problem depends upon placing the block under the objective and using it as a footstool in reaching.

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[3] Yerkes, R. M., *Almost Human*, Century Publishing Co., 1925.

[4] Köhler, W., *The Mentality of Apes*, Harcourt, Brace & Co., 1925.

This test situation corresponds to the situation described by Köhler on page 40 of *The Mentality of Apes.* It will be noted that instead of bananas and oranges, which Köhler used, toys are substituted throughout the two Series; the block is the analogue of the box which Köhler provided; and the children were consistently tested individually so as to preclude imitation, competition, and other complications.

Situation B. This differs from the previous situation only by the substitution of a small chair (such as is used in the playroom) for the hollow block, and corresponds to Köhler's substitution of a table and ladder for boxes, as described on page 49 of his book.

Situation C. Here the objective is removed to a shelf 4 feet high, where it can be reached by means of the hollow block, now placed five feet to the left of the objective. Köhler commonly follows the practice of removing the objective to a different place, keeping other factors unchanged.

Situation D. Here the setting of Situation A is reproduced except that the objective is hung higher so that it can be reached only by turning the block on the perpendicular side. This situation was not designed by Köhler but the device was hit upon by Sultan, the genius-ape, and by two of the other apes (see page 48 of *The Mentality of Apes*), and was therefore included in the Series.

Situation E. In addition to the hollow block in its usual place, a wooden grocery box 12 inches by 15 inches by 18 inches is placed about 7 feet diagonally to the left of the objective, which is now hung still higher so that it can be reached only if the block is piled on top of the box and the structure mounted by the subject. The grocery box was introduced here instead of another block so as to provide a wide base upon which to place the upper block, thus insuring a stable structure and avoiding mishaps and fear of mishaps. The box was also taken from the nursery school equipment. This situation is described by Köhler on page 139 of his book.

Series II

Situation A. The center of activity for this Series is a play-pen, 7½ feet long, 3 feet wide, and 2½ feet high, thus reaching to about the sternum of the subject. For this situation an attractive toy is placed outside the pen and the child is inside. Somewhat to his left lies an ordinary stick about a yard long. To procure the objective the subject must reach with the stick either through the bars or over the top of the pen. This situation is described by Köhler on page 32, the pen corresponding to the barred cage used in his experiment.

Situation B. The setting remains the same except that a toy broom is substituted for the stick. Here again the substitution was introduced by the apes themselves, using tin drinking cups, straws, blankets, etc., in the absence of sticks. (See page 34 of *The Mentality of Apes.*)

Situation C. The stick of Situation A is here placed outside the bars of the pen so that it can be reached only with the short stick (1 foot long) which lies inside the pen. The toy is placed as in Situation A but can be reached only with the long stick. The identical situation is used by Köhler and is described on page 180 of his study.

Situation D. Two halves of a fishing rod are placed inside the pen. The toy is placed farther away so that it cannot be reached with either half. If the two halves are fitted at the metal ends, the joined stick is adequate to bridge the gap. Köhler uses ordinary bamboo sticks for this experiment, which is described on page 130 of his book.

GENERAL PROCEDURE

The subjects were always tested in the morning between nine and eleven, which meant taking the child from the playroom, roof, or yard, presumably to play another game.

The experimenter fetched each child herself and usually chatted with him or her on the way to the testing-room. Upon entering, the subject was at once told what to do, though for the first Series this was hardly necessary since the situations were not only self-explanatory but even compelling, so that the subject would often "go after" the swinging objective of his own accord. The instructions, when necessary, were, "Do you see this dog (doll, bird, etc.)? See if you can get it." Or, "Yes, that's for you," in response to the subject's look of enquiry.

The instructions for Series II were somewhat different in nature. The subject was invited into the play-pen with "Come into this little house and see if you can get the little dog (or bird, airplane, etc.) out there." For subsequent exposures it was only necessary to say, "See if you can get it today."

General Character of the Investigation

In these experiments the writer strove to present situations to the subjects which would not be serious departures from their daily life at the Nursery School or at home; which would be so stimulating as to challenge them to action without undue urging on the part of the experimenter; and which would present problems difficult and varied enough to elicit a solving process representative of children at this age level.

It is hoped that the description of the nine test-situations shows beyond a doubt that the first aim was achieved; and that the activity of the subjects in these situations, to be described in a later chapter, answers fully for the second aim. Furthermore, the fact that a large number of the subjects at each age level were unable to solve some or all of the problems after repeated and varied effort indicates that they had actually run the gamut of resourcefulness and ingenuity on these problems.

The "games" were well liked and welcomed by the subjects, with the exception of a few of the youngest and most timid ones. A solution would yield the most extravagant joy and even ecstasy, not unlike the creative thrill experienced by adults under similar circumstances. Just as Köhler's genius-ape, Sultan, repeated the activity in the stick-jointing experiment,[5] so, too, the human subjects would often repeat experiments as if for the joy of once more trying out a newly discovered and, what is more important, a self-discovered solution.

The experimenter tried to carry out Köhler's suggestion that the elements of the test-situation should be so placed as to be within sight of the subjects.[6] This was not always feasible after the first few moments of the exposure, for the subject would as likely as not turn his back on the block or the stick while reaching for the objective. Furthermore, the child is inclined to see things only on his own eye-level, with the result that the block or stick on the floor was often as invisible as though it were hidden.

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[5] Köhler, W., *The Mentality of Apes*, 133.

[6] *Ibid.*, 11.

The Record

The experimenter kept a running account of each subject's activity in a test-situation; his remarks were taken down verbatim whenever they threw any light on the solving process, the total time spent at each exposure as well as the distribution of time over various activities was kept by means of a stop watch, the number of exposures required by each subject for the solution of each problem was noted, and the subject's age was recorded. These items constituted the complete record for each subject.

In the following section representative samples of these records will be presented and in subsequent sections they will be referred to as illustrative material.

A PRELIMINARY SURVEY OF MATERIAL

TYPICAL CASES

Case 1

Subject: Boy, 40 months, II-C,[7] 17′ 45″.[8]

First exposure (4′ 15″).[9] Subject grasped the short stick inside the pen, looked at the objective and indicated by his glance that he saw that the stick was too short; tried to reach with it the long stick outside. His movements were very jerky and impatient. Subject tried to reach long stick with his hand before it was close enough to the bars; tried to climb out of the pen, leaned over the top, shouting "Stick." (1′ 30″) Subject looked about quietly; (2′) reached for the long stick with his hand and appealed to Experimenter. He was distressed and angry and tried to force his way out of the pen. When Experimenter shook her head disapprovingly, Subject shouted, "Yes!"; began reaching for objective with his hand. Subject was on the verge of tears, so Experimenter ended the exposure, assuring him that he would have another chance. Once outside the pen, Subject grabbed the long stick, placed it close to the bars and begged Experimenter to leave it there!

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[7] This stands for the situation recorded—Situation C of Series II.

[8] This represents the total time spent by the subject in a given situation.

[9] This represents the time spent at one exposure.

Case 2

Subject: Girl, 38 months, I-A, 1′—.

First exposure (1′). Subject tried to reach balloon-objective with both hands but in 15 seconds looked around and said, "On box"; on her way to the block and as she carried it toward objective, complained, "I can't get it; you get it; it's too high." Subject placed block in good alignment, got on, and reached the objective with great joy.

Same Case, I-E, 7′ 30″—.

First exposure (4′). Subject ran directly to grocery box, which she placed under objective and tried to reach up; complained and went off for block, placed it under objective, trying to reach it; jumped off almost at once and begged Experimenter to get it; (2′) seized grocery box and piled it on top of block, succeeding only in engulfing the latter. Subject appeared amused and startled, got on this pseudo-structure but jumped off without trying to reach. Disgusted and angry. Subject pulled the boxes apart and walked off. (3′ 15″) Subject piled block on top of grocery box but now the structure stood about 4 feet away from objective. Subject got on, her back to objective, and jumped off again, repeating this several times with great pleasure.

Second exposure (3′ 30″). Subject exclaimed at bird-objective, looked around and said, "You get big box; I can't get it"; (1′) went for grocery box, placed it on highest side, got on, and tried to reach objective; jumped off and said, "I can't get it"; (2′) went over to block, picked it up, and piled it on grocery box, got on, and reached objective.

Same Case, 40 months, II-D, 13′ 35″—.

First exposure (3′ 20″). Subject examined one of the sticks and tried to reach objective with it over the top of pen; examined the other stick and used it in same way, repeating, "I can't" over and over; (1′) tried out the stick between the bars, over the top of pen, finally striking it viciously against the floor; complained bitterly and tried again to reach as before, stretching and straining; (2′ 45″) tried to climb out and whined, "I can't." Experimenter terminated exposure to avoid fatigue.

Second exposure (3′ 15″). Subject reached for objective as above and in 10 seconds said, "Look, I can't," but continued her efforts; (45″) fitted sticks up against bars of pen, banged them together, etc. (2′) Subject tried to reach objective with her hand through the spaces, to force her way out, to shake the pen, etc.; (3′) said, "Dolly does not want me to get him."

Third exposure (3′). As above, complaining intermittently and finally giving up.

Fourth exposure (4′). Subject stretched for objective over the top of pen, striking out angrily with stick, complaining, and asking Experimenter to move objective closer. (1′ 45″) Subject said, "Let's try big stick on little one," picked up the other stick, examined ends carefully and succeeded in fitting them, with a shout of "Bang!" (1′ 50″) Subject angled for objective, reached it exultantly, and repeated stunt several times.

Case 10

Subject: Boy, 36 months, II-A, 4′—.

First exposure (4′). When inside the pen, Subject sought an exit, tried to climb out and said, "I can't get out." (35″) Subject looked around, leaned over for the objective, and reached for it with right hand, complaining; stuck leg through space and complained that he could not get out that way, shouting, "Open the gate!"; tried to force his way out. (3′ 30″) Subject looked around, saw the stick and pounced on it, saying, "Let me have the stick to get the bird." Subject reached the objective very skilfully.

Same Case, II-B, 15″—.

First exposure (15″). Subject cast one look at objective and swerved off for broom which he used to reach objective at once, repeating the experiment for fun.

Same Case, II-C, 6′—.

First exposure (5′). Subject picked up the little stick and said, "I want the other stick, not the little one," and laid it down, demanding the long one from Experimenter; leaned over the top of the pen, sought an exit and whined, "It's too far away"; (1′ 50″) picked up the short stick, tried once to reach objective with it, and again began to whine, "I can't get it. You give me the big stick"; looked around, repeating his complaints and then concentrated on objective again, trying to reach it by sticking his leg out, leaning over and trying to climb out.

Subject's complaints became too persistent, and the exposure was terminated.

Second exposure (1′). On the way to the room, Subject said, "I'm going to get the big stick." When inside the pen, Subject picked up short stick, at once reached long stick with it, and then reached the objective with the long stick.

Case 26

Subject: Girl, 26 months, II-A, 20′—.

First exposure (4′ 15″). Subject looked at duck-objective outside the pen and in 15 seconds tried to get her leg between bars; tried to reach objective by leaning over the top of pen, by climbing over, shaking the bars, searching for an exit and finally looking about. (1′ 25″) Subject said, "Show me how to get duck in here"; (1′ 45″) reached through the space for objective, first with one hand and then with the other; (2′ 15″) looked in the direction of stick and tried again to reach as before and looked around in a bewildered way.

Second exposure (4′ 30″). Subject looked at objective for 10 seconds and then tried to reach as above; (1′ 5″) looked around, kicked stick back to the wall and tried to reach again; asked Experimenter if she could go upstairs.

Third exposure (4′). Subject looked at objective for 15 seconds and then leaned over for it; (35″) looked around and shoved stick with her foot to the wall, then tried again to reach objective; (1′ 30″) tried to get out of pen, and finally asked to get out.

Fourth exposure (3′ 45″). Subject looked at objective for 15 seconds and then tried to get out. (30″) Subject walked off, stepped on stick, picked it up and placed it on molding extension; (1′ 20″) returned to bars, trying to reach as before; (2′) tried to get out and (2′ 30″) said, "I want to get out," and made no further attempt to reach objective.

Fifth exposure (3′ 45″). Subject looked at objective and in 10 seconds turned away and came across the stick; leaned over it as though to pick it up but checked herself and looked at Experimenter; (25″) placed stick on molding extension and then tried to reach objective as above; (1′ 45″) "I want to get out." Experimenter urged her to try again, whereupon she repeated above performance.

Case 28

Subject: Boy, 39 months, I-C, 15″—.

First exposure (15″). Subject caught sight of objective on the shelf at once, went directly for the block, placed it with an eye on the objective, got on and reached it.

Case 30

Subject: Boy, 49 months, II-A, 3′ 15″—.

First exposure (3′ 15″). Subject looked at objective over top of the pen and in 5 seconds began to look around; sat down and peered through the bars; (25″) "I'll go out and get the dog in," to which Experimenter replied by shaking her head negatively. (40″) Subject picked up stick, looked at Experimenter, and very tentatively put the stick through the space, again looked at Experimenter and drew stick in before it was half way out; placed it where he originally found it. (1′ 45″) Subject tried to reach objective with left hand through space, saying, "It's too far," but continued to stretch for it; (2′ 30″) tried to reach with stick again, succeeded and was thrilled when the toy was in his hand.

Same Case, II-D, 5′ 15″—.

First exposure (3′). Subject examined sticks and tried to reach objective with one of them, pushing it through space. (25″) "I can't reach it with this stick. I must have another big stick." With the help of one stick, Subject shoved the other out as far as it would go toward objective. He appeared disconcerted at the loss of the stick, and Experimenter restored it; (1′ 30″) stuck finger into the hole at one end of the stick and then tried to fit both sticks together. Finding this difficult, Subject tried to fit them at two other ends and succeeded in inserting one end into the other but the result was a loose, wobbly affair. With this unstable structure, Subject attempted to angle for objective by holding on with his hand at the place of juncture. The sticks fell apart and Subject said, "Ah, I can't get the duck. Tomorrow you'll have stick that holds."

Second exposure (2′ 15″). Subject tried to reach objective by putting both sticks outside the bars and said, "He's too far"; turned his back on the objective and played with the sticks against the wall; returned to objective, tried to reach with one stick, then examined both, fitted them carefully and reached for objective successfully.

Case 39

Subject: Boy, 27 months, II-A, 5′—.

First exposure (5′). Subject walked around, sought an exit, stepped on the stick, looked at it and then tried to force his way out, distressed, moaning and beating at the bars like a caged animal; tried to climb out. (2′ 20″) Subject looked at stick, picked it up and said, "What's this?"; played around with it and (3′) began to beat the duck with it, clumsily throwing it one way and another, enjoying himself tremendously. When objective was near the bars, Subject took it in.

Same Case, II-B, 2′ 15″—.

First exposure (2′ 15″). Subject stuck his leg out through the space and whined for objective; (15″) walked over to the broom but turned his back on it; sought an exit; reached for objective over top of the pen; tried to squeeze through space, and moaned. (1′ 30″) Subject picked up broom and used it skilfully for objective, saying, "See."

Case 42

Subject: Boy, 32 months, I-A, 12′ 30″—.

First exposure (3′). Subject looked up at suspended bird; appeared shy and self-conscious, looking intently at his fingers, buttons, etc.; began to look around the room, always with an eye on objective, and then to walk around in small circles, never leaving objective for more than a few seconds. During one such round Subject walked into block (his back toward objective), stopped for a second and continued around the circle back to objective; came in contact with block once more in an unseeing way.

Second exposure (5′ 30″). Subject ran over eagerly to suspended dog, looked at it and began to survey his hands; walked around objective, surveying it from every angle, and again examined his hands, showing Experimenter some scratches on his fingers; asked her to roll up his sleeves, interrupting the performance from time to time to go over to objective, talking to Experimenter rapidly about it. (During these declamations he stood directly in front of block but appeared to take no notice of it.) (4′ 45″). Subject saw block, bent down as if to take it, but checked himself; walked away and around, then returned to objective.

Third exposure (3′). Subject tried many times to reach suspended duck with right hand, saying about fifty times, "Can't, can't, can't catch duck"; came to Experimenter with the same chant and then returned to objective, reaching for it as before and at the same time looking around the room; (2′) caught sight of block, pushed it over, asking, "Stand on block?" He carefully aligned block with objective, got on, and was ecstatically happy to reach the objective. Subject wanted to know why he couldn't take objective down, and turned block on its perpendicular side in an attempt to do so.

Same Case, I-E, 13′ 30″—.

First exposure (3′ 30″). Subject surveyed the field and went directly for the block, which he placed on its perpendicular side underneath objective, got on and said, "I can't"; (1′) went for grocery box, dragged it over to objective, got on and tried to reach objective, ending with, "I can't do it," after which he ran off. (2′ 15″) Subject got on box again, reached up for objective and in a disheartened voice, said, "I can't"; jumped off, began to put on his hat and gloves, ready to quit.

Second exposure (4′ 30″). Subject was fascinated by the dog-objective and stared at it for 10 seconds, finally saying, "I can't get it up there. I can't reach it any more," and refused to try again. (45″) Subject tried to strike at objective with his hat but after two such attempts ran off to the other side of the room and refused to make another effort to reach objective.

Third exposure (5′ 30″). Subject ran off for grocery box, which he placed on its perpendicular side, eyes on objective while doing so; tried to reach objective and jumped off; turned the box 100 degrees, still on its highest side, got on again, reached up for objective and jumped off; turned box on its lower side, got on, tried to reach objective, and complained of his failure. (1′ 45″) Subject ran away and began to play a little game of his own. When Experimenter urged him to try again, Subject emphatically told her he could not reach the objective. (3′ 30″) Subject went for block again, placed it on its highest side, got on and tried to reach objective, announcing, "I can't reach it"; got off and walked away. (4′ 45″) Subject ran over to the block, placed it on top of grocery box, moved structure underneath objective, got on and reached it, overwhelmed with joy.

Same Case, 37 months, II-A, 3′ 45″—.

First exposure (3′ 45″). Subject looked at objective outside the pen and in 10 seconds tried to reach it by sticking his arm through the spaces, first the right then the left, saying after each attempt, "I can't"; sought an exit, tried to climb out, stuck his leg through the space, tried again to reach with his hand, saying over and over, "See, I can't." (1′ 45″) Subject looked around in a distressed and unseeing way in the direction of the stick; (2′) reached again as before, sought an exit, asked Experimenter to move objective closer; (3′) looked in the direction of the stick, pounced on it, and carefully moved objective with it to where he could reach objective with his hand.

Same Case, II-B, 40″—.

First exposure (40″). Subject grasped broom at once and reached objective; repeated performance for fun.

Same Case, II-C, 4′ 15″—.

First exposure (4′ 15″). Subject tried to reach airplane with short stick and then tried to climb out; made one attempt to reach long stick and then, aiming at objective, threw the short stick out at it, not viciously, but merely to establish contact with objective, it would seem. Experimenter casually replaced the stick. (1′) Subject pleaded, "May I have the other stick?" pointing excitedly at long stick; threw short stick out at long stick, and Experimenter once more returned it to the pen. (2′ 15″) Subject reached the long stick with short one and then reached objective with the former.

Same Case, II-D, 3′—.

First exposure (3′). Subject reached for objective with both sticks over the top of the pen and in 10 seconds said, "I can't reach it," repeating the complaint over and over; (25″) reached for objective with one stick over the top, striking out viciously and finally throwing stick at objective; (1′) reached for objective with his hand through space, stretching to the utmost and complaining; (1′ 30″) appealed to Experimenter; (1′ 45″) picked up the other stick, with which he drew in the first, examined them, tried fumblingly to fit one into the other. He interrupted his performance with frequent looks and jerks toward objective. Subject began to redden under the strain, and Experimenter helped to join the sticks by holding them at the middle so as to steady them.[10] Subject reached for objective at once.

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[10] Experimenter took the liberty to assist here and in other cases where the solution was so obviously consummated in Subject's mind, as borne out by his gestures and his behavior in general. The difficulty in such cases is

Case 43

Subject: Girl, 32 months, I-A, 2′—.

First exposure (2′). Subject dashed at suspended duck, right hand outstretched, not giving Experimenter a chance to remove her hat and coat. When Subject found she could not reach objective, she began to rip her things off. That accomplished, she immediately picked up block, placed it directly under objective, and snatched at objective.

Same Case, II-A, 4′ 45″—.

First exposure (3′ 30″). Subject reached with her hand over the top of the pen, shook the bars, stuck her leg out through the space, sought an exit, again shook the pen, tried to reach objective with her hand over the top of pen, leaning over as far as she could, repeating in rapid staccato, "I can't get it"; looked around, directly at stick, at objective outside the pen, at Experimenter. (2′ 30″) Subject reached as before and looked around bewildered.

Second exposure (1′ 15″). Subject stuck her leg outside the bars, shook the bars, and in 15 seconds picked up the stick and said, "Stick"; waved it tentatively outside and then suddenly began to angle for objective as though the idea had just occurred to her.

Same Case, II-B, 25″—.

First exposure (25″). Subject used the broom for objective at once and deftly.

Case 44

Subject: Girl, 32 months, II-A, 40″—.

First exposure (40″). Subject looked around and in 5 seconds turned upon the stick, grasped it and clumsily worked it over top of pen until objective was close enough to reach with her hand.

ANALYSIS OF MATERIAL: THE RESPONSE

From the point of view of the subject, his activity in the problem-situations should be considered as a continuous experience, beginning with the first sight of the objective and ending naturally with the solution of the problem or abruptly with the

a purely motor one and, in fact, partly due to the eagerness to try out the solution which appears to be already clearly defined in Subject's mind. Where this was not so obvious, Experimenter did not render the service.

last exposure in the absence of a solution. From the point of view of the problem, however, the subject's activity may be divided into his first response to the problem-situation, which may and may not be synonymous with the solving process, and the solution itself. An analysis of the response is here undertaken.

The children's responses to problem-situations varied widely, as widely as their personalities. Yet it is by no means impossible to make some classifications. The first and most obvious classification that suggests itself is on the basis of the above implication that the response and the solving process are not necessarily synonymous. Thus we would have in one small group those subjects who made no or almost no attempt to solve. Their response was either weeping at being removed from the playroom, or silent, sometimes sullen, watching of the objective. In the other group would fall all the other subjects who made a more or less consistent effort to get the objective. The other cases cited in the preceding section are illustrative. The frequency of each class is as follows:

1. No attempt to solve (Negative Response) 4 children, or 25 per cent.
2. Definite attempt to solve (Positive Response) 40 children, or 75 per cent.

The ages of the children in Class I are 19, 23, 27, and 28 months respectively. Though among the youngest, they were not actually the youngest, with the exception of Case 19 who was 19 months old. Chronological age is therefore hardly enough to explain this unresponsiveness.

The mental ages of these subjects (derived from Kuhlmann-Binet tests) are 20.4, 30, 31.5, and 33 months respectively. Again though among the lowest, they are not actually the lowest except Case 19, 20.4 months. Subjects with mental ages lower than the other three not only made definite attempts to reach the objective, but actually solved one or more problems. Besides, the correlation between mental ages and solutions is very low, only +0.266. Thus mental age, likewise, is not an adequate explanation of unresponsiveness. Nor can we satisfactorily explain unresponsiveness on the basis of interest for we would still be without an explanation of the absence of interest.

It appears more than likely that the failure of these four subjects to respond positively to the toy-reaching situation is due to immaturity, both social and emotional. Just as children of the same chronological age may vary in physical and mental development, so they may also vary in their social and emotional development. These four children were loath to leave the room to which they had become accustomed; they were apprehensive about entrusting themselves to the experimenter, and their attitude toward the situation in the examining room was highly colored by this displeasure, more or less actively expressed. It may therefore be said that these subjects were responding primarily, not to the test-situation but to the change in routine. This may fairly be called a socially and emotionally immature response.

Another classification suggests itself on the basis of directness of Response. Into the first class would fall those children whose first reaction is an overt, unmistakable attempt to reach the objective, as Case 43, I-A. For these subjects the situation is so compelling as to make instructions hardly necessary. Into the second class would fall those who do not so commit themselves to action, but rather stand off, observe the objective and talk about it or some irrelevant matter before attempting to reach for it, as Case 42, I-A. (It is of interest to note that Cases 42 and 43, representing antithetical behavior, are twins.) It can hardly be said that for these subjects (except the four mentioned above and included here) the situation as a situation is less compelling, but rather that they cannot so readily forget themselves. The frequency for these two classes is:

1. Direct response ....30 children, or 68 per cent.
2. Indirect response ....14 children, or 32 per cent.

This division suggests Jung's two classes, Extravert and Introvert, the former responding readily to his objective environment and the latter "interposing a subjective view between the perception of the object and his own action"[11] which serves inevitably to inhibit direct action. Such a classification is, however, better designed to throw light upon types of personality than upon types of response.

If we are now to consider the response as synonymous with the solving process, that is, as an actual attempt to reach the objective, we find that four distinct types stand out:

1. *The Primitive Response.*—This refers to the act of reaching for the objective with the hand. Such a reaction is inherent

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[11] Jung, C. G., *Psychological Types*, Harcourt, Brace & Co., 1926.

in the situation and is consequently often merely the forerunner of other responses. For example, it often happens that the child reaches up for the objective immediately upon entering the room, without having had time to see what else there is. After one or two such attempts, the child does look about, sees the block or stick, and uses it correctly at once. Or it may happen that the child who has already learned to use the stick or block will reach for the objective with his hand once or twice, as if in spite of himself, and then use the stick or block. Again, it may happen that a child who has already discarded the Primitive Response as a means of reaching the objective, will, when faced with a dilemma—an insoluble problem—revert to the Primitive Response. It is because of this prepotent nature of the response, overdetermined as it is by both the situation and the subject's customary reaction to such a situation, that it is here called the Primitive Response. The word as used here has no phylogenetic connotations, hardly any intended ontogenetic connotations, though the reaching reaction at the sight of an object does happen to be one of the earliest in the development of the child, occurring as it does about the sixteenth week. Cases 43, I-A; 42, I-A-3 [12]; 42, II-D; and 30, II-A are illustrative of the different varieties of this type of Response.

2. *The Random Response.*—Objectively this corresponds almost exactly to the reaction of Thorndike's cats when confined in the puzzle-box:

"When put into the box the cat would show evident signs of discomfort and of an impulse to escape from discomfort. It tries to squeeze through any opening; it claws and bites [13] at the bars and wires; it thrusts its paws out through any opening and claws at everything it reaches; it continues its efforts when it strikes anything loose and shaky." [14]

On the whole, the desire to escape confinement plays only a minor rôle with children in these situations, the major concern being to get the objective upon which attention is riveted throughout the experiment, as is plainly judged by the facial expression, by the repeated attempts to reach, and by what the subject says. One is therefore justified in saying that though the external picture of the subject in this situation often resembles that of the cat in the box, the underlying cause of such behavior is more resentment at being thwarted than distress at confinement and is in this respect comparable to the outburst of chimpanzees under similar circumstances.[15]

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[12] The figure 3 refers to the exposure.

[13] This does not hold true for the subjects in the present study.

[14] Thorndike, E. L., *Animal Intelligence*, Macmillan, 1911, 35.

The emotional tone and tempo of activity is the characteristic feature of this response. The subject is either distressed, or violent. The solving process in any case is carried on jerkily and impetuously. The Random Response in children may occur at the outset of an experiment or after the subject has made other futile attempts to reach the objective.

3. *Exploration and Elimination.*—This refers to a deliberate trying out (again on the basis of emotional tone and tempo of activity) of one possibility after another or an investigation of the constituent parts of a situation. Sometimes this Response is guided by a partial understanding of or insight into the situation, but more often it is of a lower order, guided only by the subject's desire for the objective out of reach.

4. *Immediate Solution.*—This Response is exhibited where no apparent problem seems to exist, as in Cases 28, I-C and 44, II-A. Ordinarily it occurs at the outset of the exposure, as its name implies, but included in this class are also those solutions which are preceded for a brief interval by the Primitive Response allowing for a period of orientation. Such an allowance is defensible in the light of what was said about the Primitive Response.

Several facts are disclosed by a tabulation of the four types of responses.

First, that the response fits the situation. Thus the Primitive Response is less and less frequent as the test-situations and the idea of "tools" become more and more familiar. There is a drop in frequency after the first situation of each Series. When it occurs at all in subsequent situations it is a temporary reversion to a prepotent response or gesture due to momentary forgetfulness or to a lapse in resourcefulness, as when the subject is face to face with a dilemma. Random Responses in Series I are infrequent as compared to Series II, where a semblance of confinement and a greater number of details are introduced into the test-situations. Only two subjects exhibit this response in Series I, and even in these cases the term is more descriptive of the emotional tone than of the method. As one might expect, the fall and the rise in frequency of the Exploration and Elimination Type of Response conform directly with the fall and the rise in the difficulty of the test-situations and inversely with the frequency of Immediate Solutions.

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[15] Köhler, *The Mentality of Apes*, Harcourt, Brace & Co., 1925, 48.

Second, that a subject does not consistently adhere to one Type of Response but varies it in accordance with the needs of the situation. This may be regarded as a corollary of the first observation.

Third, that Exploration and Elimination is the most frequent Type of Response, the Primitive Response is a close second, and the Random Response is relatively very infrequent.

ANALYSIS OF MATERIAL: TYPES OF SOLUTION

The Immediate Solution which has already been considered as one Type of Response may now serve as an introduction to the question of Types of Solution. The Immediate Solution, we have seen, comes as a result of complete and immediate insight into the problem, that is, where no problem actually exists for the subject.

Where the solution is not immediate, insight is evolved in the course of the solving process in one of two ways: gradually, when it is preceded by the subject's betrayal of his awareness of the presence of the block or stick; or suddenly, when it occurs unpreceded by his betrayal. Such betrayals of awareness may be expressed by a long hard look at the block or stick, by kicking it, by carrying it around, by changing its location, by shifting its position. As a matter of fact these contacts between the subject and the block or stick often indicate more than mere awareness of its presence. The subject seems to sense a cue; thus these contacts are, in many cases, glimmerings of insight. However, one is not obliged to commit oneself to this interpretation.

It is obvious, of course, that a subject may be aware of the presence of the block or stick without betraying it by his behavior, and when he finally uses the one or the other correctly and with insight, this insight seems to have emerged *suddenly*, unheralded by betrayals, without warning, as it were.

The reader's attention is directed to the fact that the word *sudden* has no reference to the "rush" with which insight usually comes to a head, whether gradual or sudden in its onset. This "rush" or "flash" or "jerk" quality (these are only a few of the terms used by writers to describe insight) is inherent in the nature of insight.

Nothing has thus far been said about the degree of insight accompanying a solution. Yet the writer cannot assert with Koffka and Köhler that ''an appropriate transformation of the field precedes the objective solution''[16] invariably. Though this is true of most solutions, there are cases of which it is more accurate to say that ''an appropriate transformation of the field'' occurs coincidentally with the solution, possibly continuing after the act of solution is completed. It may be said of these solutions that they are accompanied by *partial* insight, gradually evolved, as contrasted with solutions accompanied by *complete* insight, illustrated by Case 43, II-A, already referred to. Though insight is here evolved gradually during the process of Exploration and Elimination, there can be no doubt that when the subject begins to reach ''as though the idea had just occurred to her,'' she does so with *complete* insight. An almost tangible break occurs when she ceases to wave the stick outside uncertainly and reaches for the objective with assurance. The ease with which the basic principle of Situation A is seen to be carried over to Situation B is further proof that complete insight was present when the solution occurred.

For contrast let us now examine Case 39, II-A, which illustrates a solution with *partial* insight. Here again insight matured gradually, but, judged by the Primary Criteria of insight, it remained partial even after the solution. At the point where the subject began to beat the duck, there was no definite break in attitude. To the experimenter it appeared to be a continuation of his play with the stick but more exploratory and better defined. When the subject saw the duck at the bars his expression still showed wonder as much as enlightenment. If we now turn to his performance in Situation B, only four days later, we find that a solution must be worked out almost anew. Note, however, the skill and precision with which the broom was finally used—an indication of complete insight.

The discussion of Types of Solution may be concluded with the following classification:

1. Solution with *Immediate* Insight.
2. Solution with *Gradual* Insight.
    - A. *Partial* Insight.
    - B. *Complete* Insight.

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[16] Koffka, K., *The Growth of the Mind*, Harcourt, Brace & Co., 1925, 205.

3. Solution with *Sudden* Insight.
    A. Matured *during* exposure.
    B. Matured *between* exposures.

COMPARATIVE DATA

Because of the fact that Köhler's experimental work was limited to nine apes only it is impossible to derive quantitative indices of comparison between their performance and that of the children who served as subjects for this investigation. The comparative data must therefore be limited to qualitative facts which, it is hoped, will nevertheless throw some light upon striking similarities or dissimilarities in the behavior of apes and children when confronted with the same (objectively speaking) problem-situations.

TYPES OF SOLUTION

The Immediate Solution. This does not occur in key-situations among apes as it does among children, but is confined to Situations B and C of Series I and Situation B of Series II. Nor does it invariably occur in these situations. It is impossible to determine how the frequency of the Immediate Solution in these situations compares for the two types of subjects, though it is probably higher for children, since an ape's solution is often delayed by his difficulty in seeing the connection between stick or box and objective unless these are so placed that he can see both at the same time.

"If the experimenter takes care that the stick is not visible to the animal when gazing directly at the objective—and that, *vice versa,* a direct look at the stick excludes the whole region of the objective from the field of vision—then, generally speaking, recourse to this instrument is either prevented or at least greatly retarded, even when it has already been frequently used."[17] Though this handicap is to some degree true of children in the key-situations, it is not all true in subsequent ones, when they have become familiar with the use of "tools." Here it no longer matters in the least whether the child is looking at the stick or box and objective at the same time or not and most often it does not so confront them. This points to a higher level of transfer and retention in the child, while the absence of Immediate Solutions by apes in the key-situations indicates either a lower level of mental development or a narrower range of experience.

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[17] Köhler, W., *op. cit.*, 38.

Solution with Gradual Insight. This is perhaps the most frequent type of solution among apes, whereas the Immediate Solution is most frequent for children. Gradual solutions with partial insight are absent except for the problem of statics in the building experiment (I-E). It is significant that this problem barely exists for children. The stability of the structure offers no difficulty once the idea of the structure is grasped. Only in three cases was the grocery box placed on top of the smaller block, and this structure would have been sufficiently stable had not the open side of the grocery box faced the floor, thus engulfing the smaller block! This was hailed by all three subjects as a huge joke and led in one case to the more foolish procedure of piling the green block on a corner of the open face of the grocery box, with the natural result that the block fell into the grocery box. By this time the subject was too hilarious to proceed intelligently. In fact, it seemed to the experimenter that the second error of judgment would not have occurred if the subject had not already been partly demoralized by the first mishap. In the second case the subject automatically mounted the pseudo-structure and reached up for the objective, as though under compulsion to carry out to completion the course of action originally undertaken. In the third case further exploration and elimination followed, but not in building.

With the exception of these three cases, building presents no problem to the children, even those as young as two years and eight months (though Koffka places this achievement at the end of the third year),[18] and in no case is the problem as serious as it is for the apes:

''Here are two problems: the one (put the second box up) is not really a difficult task for the animals, provided they know the use to which boxes can be put; the other (add one box to the other, so that it stays there firmly, making the whole thing higher) is extremely difficult. . . . In the latter case a limited body of special shape is to be brought into contact with another one like it, in such a way that a particular result is obtained; and this is where the chimpanzee seems to reach the limit of his capacity.''[19] The child appears to be nowhere near the limit of his capacity with regard to this point.

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[18] Koffka, K., *op. cit.*, 306

The ease with which a perfectly good idea, in fact the solution itself, is abandoned (and sometimes never again returned to, if not followed by attainment of the goal, as in two of the cases cited above and in Case 1, II-C, Series II) deserves further mention. This is as characteristic of apes as of children, and bears witness to the fact that for both types of subjects insight may occur unaccompanied or only partially accompanied by consciousness, in which case it is of a fleeting and unstable nature unless reinforced by a practical success. This is further borne out by adult introspection.

With further reference to solutions with Partial Insight, it may be noted that though there is no positive evidence that such solutions occur among apes except in the problem of statics, occasional difficulties in Situations B and C of Series I and Situation B of Series II point to the probability that they do. At any rate, the twelve instances of such solutions among children, already discussed, furnish sufficient grounds for believing that solutions may be accompanied by varying degrees of insight.

Solutions with Sudden Insight. This occurs quite commonly but there seems to be no instance of insight maturing away from the field of action and being suddenly employed when the animal is confronted with the situation afresh. It is not quite clear from Köhler's account whether the chimpanzee is incapable of delayed solutions of this type, though this seems highly probable in view of the difficulty he experiences in keeping objective and tool in mind together when the two are not physically in connection. Of course it is not at all certain what passes in the child's mind during the interval between repeated failures and the exposure at which the solution with sudden insight occurs and what relation it bears to this solution: whether the child copes with a mental image of the situation while away from the actual situation; or whether the interval merely serves to clear the subject's mind of all associations and assumptions relating to the problem so that he is enabled to see the elements of the situation in their proper relation at the next exposure; or whether the child has received aid from an adult during the interval.

Ruger regards the second explanation as more applicable to his cases of ''fixed assumptions'' which, he finds, were often broken up by just an interval after which the puzzle would be solved almost immediately upon coming back to it.[20]

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[19] Köhler, W., *op. cit.*, 152.

Chance

Though it (chance) may aid in solutions, it does so only if it serves to arouse insight. Thus, chance may have favored an optimum position of the two bamboo sticks for Sultan but insight was essential not only to help him realize that the position was optimum (which is a problem in itself), but also to see the relation of the lengthened stick to the rest of the field.[21] It should be emphasized that what has been said of chance applies only to situations such as were used for this investigation; that in situations approximating the wire-puzzle or puzzle-box type it may be the sole agent in a solution. Thus the subject, by continued random manipulation of a wire-puzzle, may actually cause it to fall apart, much to his surprise; or the cat, excitedly dashing around in the puzzle-box, may accidentally hit against the device which holds the door, thereby releasing it. These acts are solutions in an objective sense primarily, subjective only to the extent that such an experience can be utilized in subsequent solutions. And the extent to which this can be done is commensurate with the degree of insight accompanying the solution.

Failures

An analysis of solutions in terms of insight should logically be followed by an analysis of failures in the same terms. To say, however, that these are due to an absence of insight is to state only a partial truth that has not even the merit of being a fundamental one. There are grounds for believing that failures may occur even where insight is present, and the question inevitably arises: What factors prevent the arousal of insight and its natural consummation in a practical success?

The answer to this question is perforce tentative and speculative, based as it is upon subjective observations. There can be no doubt, however, that the following factors, submitted as responsible individually or in combination for the partial or complete inhibition of insight, do affect the solving process.

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[20] Ruger, H. A., "The Psychology of Efficiency," *Arch. of Psychol.*, No. 15, June, 1910, 29.

[21] Köhler, W., *op. cit.*, 313 ff.

1. *Self-consciousness.* Unable as the self-conscious subject is to lose himself, the attention directed to the problem is necessarily at a lower level. The uneasiness which is a natural concomitant of self-consciousness prevents the subject from carefully surveying the field. He is disinclined to commit himself to action lest it result in failure, of which he is afraid and ashamed. Such an emotional state imposes so many motor inhibitions, not to mention the mental inhibitions, that even where insight is present it is prevented from consummating in action. There can be no doubt that the subject sometimes entertains an hypothesis with regard to the block but this is not acted upon, because of the inhibiting force exerted by self-consciousness.

2. *Lack of Confidence.* This is generally found in combination with a general timidity and a need for adult approval and is given freer play in the presence of the experimenter. The subject prefaces all action with a look at the experimenter—manifestly a plea for approval or encouragement—and is inclined to interpret the absence of a response as disapproval or permission withheld. It is impossible to say in how many cases a flash of insight—as when the subject looks significantly at the objective, then at the box, and again at the objective, ending with an appealing look at the experimenter—is kept from maturing into a solution, on account of lack of self-confidence, but there is sufficient evidence to prove that such cases do occur.

Köhler found it necessary on occasions to withdraw behind a screen, not because his subjects, the chimpanzees, are lacking in self-confidence, but because they too tend to rely upon the experimenter, and any such reliance interferes with the maturing of insight in that it directs attention away from the field of action, *i.e.*, the problem.

This procedure was not followed in this investigation, first because it would have interfered with the constancy of objective conditions, and second, because it was felt that the withdrawal of the experimenter would have only substituted one emotional disturbance for another.

Failure in eight cases can be accounted for by a lack of confidence and its concomitants.

3. *"Fixations."* It is a truism that a problem is approached by a subject with a certain "mind set," even with certain assumptions, that further assumptions are formed with regard to it during the solving process, but to the extent to which we permit any one assumption to usurp our attention to the exclusion of

all others, we lessen the chances of seeing the problem in the right perspective and therefore of gaining insight into the problem. Thus Ruger found that his subjects were often rendered insensitive to variations by becoming too devoted to certain assumptions, no matter how accidentally adopted. He found, however, that these assumptions were often broken up by a fresh start, as on a new trial.[22]

That an ''assumption'' may be of a specific nature is borne out by Case 18. This subject solved all the problem-situations of Series I promptly and intelligently but failed to solve Situation A of Series II at the end of five exposures, merely looking in the direction of the stick and appearing very much distressed over her failure. The writer later learned from the subject's mother that it even disturbed her sleep. Questioning the mother further, the experimenter learned that the child had been in the habit of riding astride a yardstick at home, much to the annoyance of her mother who used it constantly for her work. The child was forbidden to use the yardstick. When, about the same time, she complained to her mother that she could not reach the ''doll outside the little house'' at school, her mother asked whether there was anything inside the house with which to reach, whereupon the child answered explosively, ''Yes, a yardstick!'' The ordinary stick used in this situation was actually of about the same proportions as a yardstick which apparently had for the child but one association, *Verboten.*

That associations or assumptions can and do play such tricks with our thinking and acting behavior is, of course, well known. The feature we are stressing in the above ''fixations'' is their persistency, destructive or inhibitive of insight.

This phenomenon was not observed among chimpanzees, though it should be stated with regard to the comparative data on Failures that it is inadequate throughout, on account of Köhler's practice of frequently varying the situation after an initial failure. In this way or by means of clues, advisedly administered by Köhler but nevertheless unfortunately so for the purposes of this investigation, the solving process was often interfered with.

4. *Lack of Interest.* This is often a chronic lack of enthusiasm on the part of the subject, causing him to ''give up'' too readily. Such an attitude is naturally not conducive to optimum or sustained attention, nor consequently to insight. Failure in seven

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[22] Ruger, H. A., *op. cit.*, 16.

cases may be attributed to this cause. In experiments with animals this factor is efficiently counteracted by causing the animal to undergo a preparatory fast, by heightening the attractiveness of the objective, or, as was often done by Köhler, by introducing the competitive element. Though none of these devices was used for the subjects of this investigation, the situations themselves were found to be sufficiently compelling to arouse an active interest except in these seven cases.

5. *Discouragement.* Three subjects were too easily distressed by failure. Sulks and tears, in any case a withdrawal of attention and cessation of activity, inevitably cut out the possibility of insight and hence of solution. Under similar circumstances the chimpanzee was whipped up to further action by introducing the competitive factor or by improving the objective. A verbal appeal was occasionally tried with the children, with only indifferent results.

6. *Excitability.* When the solving process is too eager, or energetic or excitable, attention is not directed to any one thing at a time, with the result that excellent "clues" are often unheeded. It is an emotional state unfavorable to insight and is the cause of failure in seven cases.

7. *Lack of Observation.* This is present in all degrees, from a total unawareness of surroundings, including the objective, to its extreme opposite, a minute investigation of all details. The extreme form of this inobservance is usually present only at the first exposure, due in part to a sudden change in surroundings, and gradually wears off to some extent. In two cases, however, this tentative, superficial, partly diffident attitude toward the changed surroundings was maintained through the five exposures, interfering with the arousal of insight. The chimpanzee, on the whole, has as strong a tendency toward exploration as has the child, but at times it, too, would "look without seeing."

8. *Emotional Immaturity.* This presents a picture of inactivity, almost immobility. Under the circumstances insight is surely interfered with, if not completely inhibited. Failure in four cases can be attributed to this factor.

If, then, we are to classify Failure on the basis of insight we can say that it may occur when insight is absent, or when it is but vaguely present or when it is partially present, *i.e.*, not extending over the whole situation, or when it is probably complete but not accepted or acted upon because of inhibitions. However, as we have seen in the foregoing analysis, failure can be expressed in terms of insight only if such an expression carries with it the conditioning factors of insight. These we have seen to be of a mental, emotional, and temperamental nature.

SUMMARY AND CONCLUSIONS

No attempt is made to summarize this investigation, for to do so adequately would require no less than a restatement of it, step by step as it was built up. In lieu of this, the following sections are presented by way of review: Quantitative Data, Conclusions, Practical Applications, and Suggestions for Further Research.

Quantitative Data

Total number of possible solutions........................ 248
Actual number of solutions..................179 or 72 per cent.
Total number of failures.................... 69 or 28 per cent.
Average number of exposures required to solve each problem-situation:

Series I			Series II		
Situation	*Av. No. of Exposures*	*Per Cent. of Cases*	*Situation*	*Av. No. of Exposures*	*Per Cent. of Cases*
A	2.25	57	A	1.52	66
B	1.09	100	B	1.08	100
C	1.09	95	C	1.40	80
D	1.13	68	D	1.77	50
E	1.72	48			

Youngest subject reaching solution of Situation I-A..23 months
Youngest subject reaching solution of Situation II-A, 24 months[23]
Oldest subject failing in Situation I-A...............46 months
Oldest subject failing in Situation II-A.............49 months[24]

Conclusions

1. A child's solving activity was found to be determined more definitely by the nature of the problem-situation than by any one other factor. Thus, the same child will attack a problem in a random trial and error fashion or in a more deliberate exploration and elimination manner, depending upon the set-up of the problem. A semblance of confinement and a totally new or bewildering situation were found to stimulate the first type

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[23] This is the youngest child used for this situation.
[24] This is the oldest child used for this investigation.

of response, while in the absence of these factors, the second type of response was the customary one. This is not to deny the fact that some children are more easily stimulated to the first type of response than others. The personal element functions here as elsewhere but not so potently as the situation itself. In its general statement this conclusion is equally true of apes.[25]

2. Exploration and Elimination was not only found to be the most frequent response but it also yielded the greatest number of solutions, alike for children and for apes.

3. No matter what the type of response, it culminates in a solution only if the subject has gained insight into the problem-situation. This, again, is equally true of both types of subjects.

4. Though chance may aid the arousal of insight by throwing the elements of a situation into a suggestive constellation, no solution was found to be directly caused by chance.

5. It is also highly probable that insight is not always complete and stable before the solution, that is, that there are varying degrees of insight accompanying a solution; but no solution was found wholly unaccompanied by insight. There is no direct evidence that the first part of this conclusion applies to apes.

6. Transfer and retention seem to be indices not only of the presence of insight but also of the degree to which it is present. This needs to be further tested by an investigation specifically designed for the purpose.

7. The arousal of insight and its consummation in a practical solution are favored by emotional, temperamental, and mental factors—those which in effect constitute the total personality. This is true in a far lesser degree of apes, probably because of their lower level of development.

Practical Applications

On the basis of the findings discussed in the foregoing pages, we submit the following recommendations for educational procedure with the pre-school child:

1. Learning situations should be so formulated as to be interesting but not too stimulating, for the former type is more likely to arouse a solving approach conducive to the arousal of insight.

2. Analogous learning situations, where the greatest amount

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[25] These conclusions, when applied to apes, are based upon Köhler's experiments reported in his book, *The Mentality of Apes.*

of transfer can be expected, are a useful check on the degree to which understanding or insight is present.

3. Retention of the principle learned in a given situation is another such check.

4. In view of the unstable nature of insight, especially when it matures after a prolonged struggle, it is wise to follow up the solution with one or more repetitions of the learning situation.

5. Some effort should be made with regard to self-conscious children to direct their attention specifically and actively upon the problem and away from themselves and the experimenter.

6. Children lacking in self-confidence and children over-reliant upon adult approval should be actively encouraged to try out all possible approaches to a problem.

For both types of children the important thing is to break up the paralysis which results from uneasiness and insecurity, an emotional state which is neither favorable to the arousal of insight nor to its natural consummation in a practical solution.

7. Children should be taught the habit of varying their solving procedure at a very young age. Such interference should, of course, be judiciously applied and only with those children who show a tendency toward unwarranted persistence at one aspect or feature of the problem. This persistence interferes with the seeing of the problem as a whole and hence with the arousal of insight.

8. Too frequent variation of the solving procedure is similarly to be discouraged, for it, too, interferes with the arousal of insight by the scattering of attention. Such solving habits are also wasteful of energy and usually compel an early cessation of activity due to fatigue or discouragement.

9. Children who show a tendency to become easily discouraged require an individual presentation of the problem where at least partial success is possible early in the period of endeavor. Discouragement at failure not only makes insight impossible in a particular situation, due to a lack of participation, but it tends also to color the approach to subsequent problems.

10. Children who lack a fair share of the tendency to explore should be frequently presented with situations which stimulate that tendency, for it greatly enhances the probabilities of arousing insight.

Though these recommendations are primarily for educational purposes, their applicability to the wider problem of mental hygiene is unmistakable. The test-situations used for this investigation furnish an excellent approach to a study of the individual as a whole, and the factors which were found to affect the arousal of insight are in effect those which constitute the total personality.

Further Research Suggested By the Present Investigation

1. The Primary Criteria of insight are those which constitute the solving attitude of the subject, whether tentative or assured. Though of a subjective nature, they are none the less of value and of a really specific nature. To use these data more objectively, it would be advisable to have several observers compare notes on the total picture presented by a subject solving a problem-situation.

2. Retention and Transfer may be put to the test as secondary criteria of insight by comparing the degree of Retention of a solution after a given interval on the part of two equated groups, the one having solved spontaneously and with insight, as judged by the primary criteria and the other having been shown the solution after an initial failure. Similarly for transfer, results on situations subsequent to the key-situation would be compared for both groups.

3. The degree to which the presence of the experimenter stimulates the self-attentive attitude, thereby inhibiting the maturation and consummation of insight, can be readily tested by subjecting two equated groups to the same test-situations, one in the presence of the experimenter and the other with the experimenter seeing but not seen.

4. It is probable that Retention and Transfer are favored by spontaneous solutions more than by taught solutions, probably because of the fact that insight is more complete and stable in the former than in the latter case. This can be further tested by permitting the one of two equated groups to solve a key-situation spontaneously and the other to enact the solution only after a demonstration. The results of retention and transfer to subsequent situations are then to be compared for the two groups.

GLOSSARY

Chronological Age, physical age; number of years actually lived.

Field, (as used) the total situation perceived where one detail is observed in its relations to other details; when an object is seen in changed or new relationships, it means that the entire field has changed, hence field is a configurational phenomenon; pertains to the subject's manner of perceiving, not to the external objects as such.

FIXATION, (as used) persistent use of one idea or assumption to the exclusion of others; prejudice or bias.

MIND SET, idea-in-mind controlling the manner of perceiving an object or situation; bias; mental set.

OBJECTIVE NATURE, pertaining to objects observed; refers to objects as things external to "mind"; outside of experience.

ONTOGENETIC, pertaining to the life history of an individual; contrasted with phylogenetic which pertains to the ancestral history of the individual in the evolutionary scale.

PREPOTENT NATURE, predisposed to occur; greater readiness of occurrence; inherited rather than acquired.

RETENTION, remembering; condition (in the brain) making possible the recall of previous experiences.

STERNUM, breastbone.

SUBJECTIVE NATURE, pertaining to mental processes as such; "within the mind"; pertaining to the process of observing, or feeling, as such; within experience.

TRANSFER, adequately responding to new and different situations when details have changed but relationships have remained constant; response involving the application of a principle.

Verboten, German word meaning forbidden or "keep off."

GROUP III

EMOTIVE BEHAVIOR

READING NINE[1]

A CASE OF DUAL PERSONALITY[2]

Editor's Note

The Configurational Character of Emotion.—In accordance with the plan of arrangement of these readings, the human being was first studied in his natural, unmolested environment, and it was evident that the outstanding characteristic of this environment was its social aspect. When the conditions of behavior were restricted to the problem-character of situations, certain features of a more specific nature emerged, namely, insightfulness and organization with respect to a goal. If the conditions are restricted further, so that only goals sought with *unusual tenacity and vigor* are considered, another feature of behavior is brought into prominence, its emotional aspect.

Every one knows in a general way what is meant by the term emotion. Love, fear, and anger are emotions. But emotion should not be regarded as a distinct category of mental processes having nothing in common with the behavior studied heretofore. On the contrary, much of emotive behavior is social in character, and all cases of it involve some degree of insight. Insofar as emotions involve insight they are modes of intelligent behavior. Emotion, therefore, is an aspect of the behavior already studied, but an aspect singled out for special investigation.

From what has just been said, it follows that emotive behavior is configurational in nature. It is a tension that demands resolution in the process of reaching a goal. Fear, for example, is a tension that exhibits itself in flight or escape toward safety; anger is a tension that is resolved in the process of conquering an enemy, overcoming a physical obstacle or removing a situation that is thwarting one's plans; love is a tension, relief from which is sought in caring for the needs of offspring or in performing a sexual act. Distress is a tension which is relieved in all manner of ways. The distress of an inferiority complex is alleviated by developing conceit and by efforts to persuade others that one is a highly important individual; the unpleasantness of cowardice is counterbalanced by an outward pose of bravery; the bitterness of a disappointment is softened by a sour-grapes attitude; the discomfort involved in admitting blame for awkwardness is offset by projecting the blame upon an outside source. So, when a person misses the ball in tennis he blames the racket, and when he bangs his head against an open door, he slams it shut as if to punish it. Modes of behavior like these are described as compensations, rationalizations, projections, and regressions, as the case may be. Daily life abounds in them.

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[1] Reading Nine parallels Chapter VII, *The Science of Psychology.*

[2] By Henry Goddard, Ohio State University. From the *Journal of Abnormal and Social Psychology,* Vol. 21, 1926, 170-191. By permission.

When normal efforts to resolve tensions persistently fail to meet with success the individual becomes maladjusted, and in extreme cases, insane. The less serious maladjustments commonly involve peculiarities of behavior, but occasionally there develops a distortion of personality so great as to require the aid of a specialist in nervous and mental diseases.

The environment that surrounds an individual ordinarily suffices in furnishing the goals for the relief of emotional tension. But there are times in the life of any normal individual when the goal is not in sight, or when it is too far distant to be reached immediately. Then, he substitutes for the real goal an imagined one which he *is* able to reach. This is the rôle that day-dreams play. Notice how often in a day-dream, the dreamer is the hero, doing big things, an avenger righting some personal wrong, a skilled performer accomplishing some extraordinary feat. Day-dreams are safety-valves, and therein lies the secret of their absorbing character. They are unitary and highly organized processes whose initial and final stages are conditioned beforehand; once they have started, nothing intrudes upon them, nothing thwarts their demand for completion, unless it establishes another goal of equal or greater importance.

But suppose the environment is so uncordial and the chances for outward emotional relief so infrequent that the individual spends much of his time in day-dreams. Then the day-dream may remove the dreamer so far from the circumstances of waking life that, on awaking, he can remember nothing of the dream. The dream has become an amnesic period. A mild situation of this kind is not an uncommon occurrence, for who has not awakened from a day-dream with a "start," unable to recall what he had been thinking about?

The situation becomes serious when, in one amnesic period, the dreamer can recall a previous period, but knows nothing of his normal waking life. Knowing nothing of normal waking life momentarily is in itself a most common experience, for no day-dreamer, absorbed in fancy, is aware of his surroundings or the circumstances pertaining to them. But let this "dissociation" last for several hours, perhaps days, and let the dissociated periods develop as an organized whole, and the individual has acquired a secondary personality.

The background for ordinary day-dreaming is an emotional life that offers no striking deviations from the normal. The background for amnesia and multiple personality is a prolonged period of intense emotional strain from which there has been no satisfactory relief.

In Reading Nine, Dr. Henry Herbert Goddard describes a striking case of multiple personality and offers a theory of its causation. A later and more thorough discussion of this case, by the same author, appears in *Two Souls in One Body?* New York: Dodd, Mead, and Company, 1927, a book which should be consulted for an elaborated and somewhat modified explanation. It should be noted that in the present article Dr. Goddard speaks of the hypnotic trance as a form of sleep; this is figurative; and the author's neurological theory of dual personality is, as he says, highly speculative.

Cases of dual or multiple personality are not so common but that a new one is of interest. Indeed we have as yet not enough cases accurately described to enable us to completely eliminate the chance elements sufficiently to have an accurate picture of the phenomenon itself.

The case to be described has some advantages over previous ones, in that the patient was younger than most cases, her early life is well known, and what are probably the significant items stand out sharply. The record during the attack, the treatment and the after history are also fairly complete.

On the evening of September 22, 1921, there came under my care a young woman nineteen years of age whom we will call Norma. She was a delicate, attractive young woman of excellent intelligence, weighing at that time one hundred and two pounds; well proportioned, apparently in good health, well mannered and in every way attractive. She was considerably fatigued from a long automobile ride and soon fell asleep. I did not see her until the next morning. As I entered the room she exclaimed, "There is Sarah." Asked her name, she replied "Polly," asked her age, she said "four years." And her behavior was consistent. She could neither read nor write, she had wrong names for the colors, she talked baby talk, was active and outspoken, crude, impatient and wilful. After a brief observation I returned to my office. An hour later I was told that she was now in her normal personality.

When I entered the room this time I was treated as an entire stranger. She looked up to see who had entered, then turned to the nurse with an inquiring look. Being introduced she said, "I am pleased to meet you." She then talked with me quietly and easily, told me something of her history, but had absolutely no memory of anything that had occurred while she was Polly. After a time she again fell asleep, and awoke as Polly. She immediately called me "Sarah," and carried on the old conversation just where she had left it before as Polly.

Late in the afternoon she fell asleep and awakened as Polly. When I entered she shouted, "Oh! there's Sarah." Although she had seen several people in the morning and given them all names Polly never made a mistake in calling them by the same name when seen on later occasions. Norma was not so successful. She is a normal young woman and sometimes hesitates when trying to name a person whom she has seen only once.

During the day, she changed personality eleven times, being Norma five times and Polly six times. The change occurred as follows: Norma, 1 hour 50 minutes. Sleep, 1 hour. Polly, 50 minutes. Sleep, 20 minutes. Norma, 3 hours 35 minutes. Sleep, 50 minutes. Polly, 35 minutes. Sleep, 35 minutes. Norma, 45 minutes. Sleep, 15 minutes. Polly, 50 minutes, Sleep, 10 minutes. Norma, 55 minutes. Sleep, 5 minutes. Polly, 1 hour 35 minutes. Sleep, 5 minutes. Norma, 45 minutes. Sleep, 1 hour 30 minutes. Polly, 18 minutes.

The transition from one personality to the other has with four exceptions always been by way of a period of sleep.

During this early period it was a very restless sleep. Later the restlessness passed off.

The procedure of her first transition is typical. After about an hour Polly suddenly dropped as though dead or in a faint; however her pulse and breathing remained normal, and she slept quietly. In a few minutes she was restless, evidently dreaming and started to get out of bed, still asleep. When held and pushed back in bed she struggled violently, requiring two people to hold her. This violent struggling lasted for two or three minutes then again she collapsed and lay as one sound asleep. After a few minutes she woke, rubbed her eyes, looked around and said politely, "How do you do?" She was Norma again.

The next morning (Saturday, September 24) she awoke at 7.30 as Polly. Slept 8.05 to 10.40 and awoke as Norma. In the afternoon I made a "professional" call upon her. She understood that I was the one to whom she had come for treatment and that I was to talk with her about herself. She answered my questions intelligently and frankly. She gave me an epitome of her illness and her early life.

At four-thirty she asked if she might get up and walk in the hall a little. She was told she might and we were sending for the nurse to come and help her dress when she fell over on the pillow. She awoke in 10 minutes as Polly. It was exactly six days before she again awakened as Norma.

Sunday the 25th saw the four-year-old Polly in all her glory though she was a good Polly. She had a baby form of speech, used "her" for she and you. "Can her do that?" She was as ungrammatical as an untaught child. Some of her expressions were "No er don't," "Yes er do," "Am I pretty?" "Do you like me?" She could not read, write or spell. She knew no letters, and had wrong names for the colors.

I had determined to try the method of automatic writing when she herself paved the way. Mr. Rausch, who has assisted me in the study of the case, had taught her the title of a magazine from a copy that was in the room. He was teaching her some of the letters when she spied a pencil in my pocket. She grabbed it—as Polly always did—and imperiously demanded in a loud voice, "Give me some paper I want to draw." I realized that here was the opportunity for my experiment in automatic writing. Mr. Rausch wrote out the alphabet for her. She copied it. Then I said quietly, "Write a letter to June" (June was Polly's name for Mr. Rausch). She did so. It was nicely written as to structure, correctly spelled and as she proceeded the writing became her normal hand. After this she wrote the names of all her family. Up to this time she did not know she had any sisters except the youngest, who was four years old. As she paused in her writing some one again asked her for a color. She gave it correctly and we found she now knew them all.

We then asked her how old she was. She replied fifteen. She tested fifteen in intelligence (as a four-year-old she tested a little under four on the Binet).

Thinking that possibly if she became nineteen, the two personalities would merge, we set to work to get her up to nineteen years of age. At first she persistently refused to be nineteen. So we contented ourselves with saying, "You are fifteen and one-half and tomorrow you will be sixteen." This she accepted and on the morrow called herself sixteen.

We were planning to proceed year by year when she herself again surprised us. *On Wednesday, September 28th,* she was writing the names of her brothers and sisters, and father and mother, with their respective ages. At this time she was calling herself sixteen years old. When she came to her brother, she put him down as sixteen, and at once commented that if he were sixteen, she could not be. She then added, "they tell me I am nineteen, so I suppose I must be." Later when she got down in the list to *Eleanor,* she said that was her twin sister, and she was nineteen, so that settled it that she, Polly, must also be nineteen. After that it seemed pretty well settled in her mind. There was, however, no change in her personality. Later it was a matter of indifference whether she was fifteen or nineteen. A few times she lapsed to four years for short intervals.

Polly was now nineteen and though somewhat more reasonable and possibly a trifle quieter was still a long way from Norma as we soon realized. As stated above, the Polly character had persisted for exactly six days when *Friday, September 30th,* at about 6.40, she awakened in her normal condition. Shortly before this I had put her into hypnotic sleep and wakened her as Norma, but she soon fell asleep and slept for some minutes, then partially wakened but without regaining full consciousness, rambled a little in conversation, looked about and was somewhat active but again went sound asleep and a few minutes later at 6.40 she awakened definitely in her normal condition. Her hand was near her head, and she felt her hair, and with a puzzled expression on her countenance said, "What is that?" She was told that the girls had put her hair up in the form in which she found it. She next noticed a ring on her finger and wanted to know about that. I told her that Polly had asked for it seeing it on Miss R's finger and Miss R had given it to her. She said, "Oh, that Polly, she does queer things." Later she remarked that she would like to see Polly. I asked her if she remembered what she was talking about when she went to sleep, and she said, "Yes, we were talking about going for a walk." This evidently referred to the time when she fell asleep the previous Saturday afternoon at four thirty. When told that that was six days ago she said, "Oh, have I been Polly all this time?" She knew that there was a Polly or that she had these spells, but had no consciousness of what they were like. Polly did not know anything about the other personality, and when asked if she knew Norma she said yes, she knew of her; asked who she was, said it was a friend of hers who was coming to see her.

For the next three weeks she was Polly most of the time, being normal only at short intervals. Then a change came and from that time on she was generally Norma with occasional lapses to the Polly condition.

The contrast of the two personalities was most marked. Norma, although rather immature for a young woman of her age—appearing more like a girl of sixteen or seventeen than nineteen—is nevertheless attractive and modest, almost diffident; very neat, of excellent intelligence, testing about sixteen on the scale and had nearly finished the first year of high school when compelled to leave on account of ill health. She is generous almost to a fault, unselfish, was devoted to her mother and brothers and sisters as long as the family remained intact; indeed so devoted that she undoubtedly worked too hard for them; is absolutely truthful, indeed so marked is this trait that even in the Polly state we have never found her falsifying anything. Her word and her promises have always been kept. She shows good taste in the matter of clothing, is polite and well mannered. All those for whom she has worked or with whom she has lived have been very fond of her and speak highly of her in every particular except when she was ill and did things for which she was not responsible. She is quick and accurate at work and after she became stronger was of real help to us in the laboratory in a clerical way.

Polly on the other hand while having many phases was in general: loud, coarse, wilful, emotional, changeable, disobedient, selfish, egotistical, excitable and unreasonable. She was sometimes a good Polly and sometimes a bad. That is to say when she was what we call good, although still wilful and excitable, she was pleasant and it was possible by working tactfully to get along with her quite comfortably. At other times this was not possible and the above characteristics showed to a marked degree. The language was rather consistently that of a four-year-old baby. She was fond of doing little stunts of various kinds and then saying to some one, "Can 'oo do that?" or frequently "Can her do that?" She would play on the floor like a small child or scribble with pencils or play with anything at hand. She would put two chairs together and crawl under them playing it was a tent. Norma was very much annoyed when told of the various things that she did as Polly. Polly was very fond of jewelry and trinkets; would beg them from anybody that came near, and would then show them to everybody that came in. She called eyeglasses "funny eyes," and anything that attracted her attention, remarking on any peculiarities of dress; and one day when a prominent lady called upon her she said, "You've got paint on your cheeks." If any one offended her she was equally free in telling them what she thought of them, frequently using language that was extremely unconventional. And at different times in her history she has been eight and ten in fact almost any age for short periods. Generally, however, she was fifteen. The fifteen-year-old Polly could be reasoned with but she was very keen at arguing especially when she wanted to have her own way. Upon one occasion when she was refused something that she wanted she declared that if she did not get it she would "act like the devil," to which I replied that it would be interesting to see how the devil would act so I thought we would try it. She promptly turned to a third person who was standing near and said, "I didn't think it would work that way." Upon another occasion she wanted to go to the dining room for her dinner and when I hesitated to grant her request she began to tease and finally said that if allowed to go that time she would never ask to go again, then instantly exclaimed, "Oh! that's too much," meaning that she had promised more than she was willing to agree to. She was very self-conscious and egotistical, constantly saying, "Ain't I pretty?", "Don't you like me?", and similar expressions. Norma on the other hand was quiet and reserved, obedient to our slightest wish, willing to stay in bed or to eat meals in her room if we wished her to do so; rather belittling her ability than otherwise, never forgetting to be polite.

Norma found it hard to understand the lapse of time when she had been Polly for several days. Polly on the other hand never seemed to understand anything about this or to realize there was anything strange in her conduct. When she waked up she took up the thread of her life where she had previously left it as Polly and any interval no matter how great never gave her any perplexity.

With the exception of a very few periods toward the end of her career Polly was always totally anæsthetic. Moreover, she was totally unaware of any peculiarity in this respect. She never said, for example, "I cannot feel anything." Pinching, pricking with needles, tickling, etc., produced not even reflex muscular twitching. A needle thrust under the thumb nail to the root of the nail elicited no response of any kind. If her eyelids were closed by approaching from behind and sliding the hand down over the head she merely said, "I cannot see; what is the matter?" She never put up her own hands to remove any possible obstruction.

At other times the loss of sensation was partial and localized and apparently motor control was also lost. For example, on two occasions she was sitting on the floor and seemed not to know that she had legs. She made no comment but though she wanted to get something out of reach, made no effort to get up or to slide along the floor. When helped up she could not stand.

Precisely the same condition prevailed at other times with eyes and ears—she heard nothing ~~or~~ she saw nothing but made no comment. These periods usually lasted only a few minutes—probably never more than a half hour.

Next to the anæsthesia perhaps the most striking phenomenon was her behavior during sleep. Her going to sleep during the daytime generally was literally a fall. If she was standing, she would fall to the floor as though suddenly struck dead. If she were sitting up she would fall over and if she were lying down there was the same sudden relaxation which generally showed by a quick drop of the head or of the arms. Then she would sleep perhaps for a few minutes very quietly although usually with a constant twitching of the fingers and mouth. After a little she would roll or toss and sooner or later throw herself out of bed unless prevented. Sometimes she would cry out uttering various expressions that seemed to indicate a very disagreeable dream. Or again she would rise and start to get up on her feet in bed. When forced back into bed she would struggle and fight, sometimes even biting and scratching until her strength seemed exhausted; then she would fall back limp and unconscious. At other times she would lie with her eyes wide open but see nothing; sometimes she would talk but would pay no attention to what was said to her; or again she might respond in a way that would seem as though she were wide awake. As an illustration of some of these features we give the following taken from our records showing her behavior for two and one-half hours, one week after her arrival:

12.55 P.M.	Asleep.
1.00	Moved limbs—breathed deeply.
1.03	Rolled on back, then on side, said "no."
1.05	"I don't want to," straightened limbs, hand.
1.06	Rolled to left side almost out of bed—mumbled.
1.07	Rolled on back, mumbled, "I don't want to."
1.10	"Ain't going to do it." Threw arms about (on back).
1.15	Pulse 92.
1.17	Moved limbs, groaned.
1.19	Rolled to left side.
1.19½	Jumped—groaned.
1.20	Stretched and relaxed naturally.
1.22½	Stretched, rolled on back.
1.24	Rolled on left side.
1.25	Struggled, said, "No, I ain't going to I ain't," opened eyes.
1.25-26	Talked, said, "I'm going to tell her."
1.27	Apparently awake; not natural; got pencil and marked on paper, said, "Look what I got." Laughed. Did not resist when pencil was re-

moved, talked incoherently, then lay still with eyes open till 1.32.

1.32 P.M. Rolled on back, said, "Ain't going to," "Did not," "Did too," several times, eyes open.

1.34 Rolled clear over on her face and went to sleep.

1.41 Rolled, struggled, said, "No, ain't going to I ain't" (on back).

1.43 Struggled—groaned.

1.45 Kicked, threw arms then slept soundly.

1.50 Brows lowered and raised, arms twitched.

1.52 Eyelids began to move, eyes parted and closed, body twitched.

1.53 Eyelids jerked, mouth partly open.

1.54 Arms jerked.

1.55 Limbs jerked.

1.57 Puckered lips as though whistling.

1.58 Rolled on right side. Drew deep breath. Asleep soundly.

2.29 Awoke.

Although much of this was very puzzling at the time, I am fully satisfied from the more complete observations of the later months that this was all different phases of sleep walking. Indeed the Polly phase itself may profitably be regarded as a more elaborate and organized form of dream consciousness or sleep walking so nearly resembling a normal consciousness as to be easily accepted for such.

Thursday, October 6 at 11 P.M. she suddenly changed from Polly to Norma without falling asleep. Mr. Rausch, who was with her at the time, notified me by phone and I came immediately from home and was with her until ten thirty, when at our request she turned over to go to sleep for the night. She fell asleep rather suddenly, to be sure, but still much more normally than usual.

Mr. Rausch's account of this transition is as follows:

"Thursday evening, October 6th, we started singing songs about 7 P.M. Sang several until 7.20. Norma was knitting. I hummed a couple of old songs in which she did not join. I was not watching her face, but soon noted that she did not join in when I returned to familiar songs. I glanced at her face and detected at once the Norma expression. I said, 'I believe you are Norma.' She did not answer but gave a real Norma smile.

Then I asked her her name and as the name 'Norma' came from her lips, I realized that she had changed from the Polly stage to the Norma without passing into a state of coma.

"In questioning Norma she said she had felt a peculiar sensation in her head but could not explain exactly how it had affected her. She recalled suggestions that Dr. Gaver had given her in the afternoon while under the influence of hypnosis. After Dr. Goddard arrived, some questioning developed the rather startling fact that we had a complete blending of the two personalities; *i.e.,* she remembered things she had done and said during her four-year-old Polly state and also many things that had happened before she came to the Bureau. She expressed sincere regrets for the things she had said and done during her four-year-old state, saying that she would have to apologize to every one in the institution for things she had said.

"She remained normal until she fell asleep at 10.30. Dr. Goddard and myself were not quite sure whether her going to sleep was natural or not. She slept well all night."

She had suddenly passed from Polly to Norma without the intermediate sleep. This is the only time it ever happened in her case, though she twice passed from Norma to Polly without sleep. Moreover, as Norma she remembered herself as Polly and what she had done. For example, Polly visited the laboratory and had been in several of the rooms. Norma had up to that time never been out of the building in which her own room was located. She now described accurately my office and other rooms in that building. All the evening she talked about her experiences like one who had returned from a foreign country.

Friday morning, October 7th, at eight o'clock she awoke as Polly. She had had a good night's sleep. When I arrived at ten o'clock she had had her breakfast and was quite in the active Polly state. I finally induced her to go into hypnotic sleep about eleven o'clock and awakened her as Norma. She was normal until one o'clock when at my suggestion given in the waking state, she laid down and took a nap. I also gave the suggestion that she sleep for an hour, and wake as Norma. She slept exactly an hour but awoke as Polly. I then tried to put her to sleep but she resisted; I finally insisted, and overcame her resistance, but the only result was that she fell into deep sleep beyond all control. After nearly an hour of troubled sleep with some calling for mother, and considerable struggling, she finally awoke as Polly. I then persuaded her to yield to suggestion and go to sleep. She finally did, not so much in co-operation with me as to show off and show Mrs. Goddard how she could go to sleep. The result showed in her wakened condition. I wakened her rather quickly and she came out as Norma apparently and actually, but on the other hand quite noticeably with the Polly character. She was not as careful of her language, that is, to speak grammatically, she forgot to say "thank you" and in various other ways showed a somewhat curious mingling of the Polly character with the Norma.

Saturday morning, October 8th, she awoke as Norma, and remained so throughout the day. She was rather tired and when asked if she would like to get up and dress, she did not care to do so. She did, however, dress and go downstairs for dinner. About the middle of the afternoon she came to the laboratory and made two or three visits among the workers. She was quiet and ladylike, like the true Norma. After a time, she asked if she might walk with the girls from the cottage, who were outside. I allowed her to do this and she went alone to her room and got her wrap. They took a very short walk and then she came into the laboratory. She sat down for a short time and then, remarking that she felt a little tired, thought she would go over to her room. I went with her and put her to sleep with the suggestion that she would sleep quietly and wake up after a while in her normal state. But while I was talking to her she seemed to transform to the Polly condition. At first I thought it was in sleep, but I soon found that she was thoroughly awake as Polly. After a time she fell asleep again, but awoke as Norma, and had her supper. After supper she fell asleep and wakened as Polly, four years old, and wanted her supper. She had complained all day more or less of a stiff neck. This gradually got worse, and remained until some time in the night when it disappeared.

Sunday morning, October 9th, she awoke as the four-year-old Polly. It did not take long however to bring her to the fifteen-year level by the method of writing. But the nineteen-year level we could not get at that time. Moreover, she was a very excited fifteen-year Polly, usually wilful. By Wednesday she had calmed somewhat and admitted that she must be nineteen years old.

Monday morning, October 10th, she awoke as Polly and was very difficult to manage all day. Whenever she slept she had violent dreams and when she was awake was very negativistic and hard to manage, insisting that she was going to be just as bad as she could. She would scream if she was crossed in the slightest degree. About the middle of the afternoon, I put her under hypnosis, and wakened her promptly as Norma. She was her normal self and stayed awake for about an hour, and then fell back, asleep. During this sleep she had some of the most violent sleep convulsions that we had seen for several days. She seemed to be living over old experiences in her dreams. She never remembers these dreams. She awoke after a time and ate her supper in the Polly condition. Later she slept again and awoke as Norma.

The foregoing will give the reader some idea of the problem we had on our hands. The early days were spent in observing her and in trying to decide upon some method of procedure. We realized, of course, that we were dealing with a problem of dissociation and that the cure of the situation would consist in bringing the two personalities together in a normal form. Three different methods were more or less definitely tried. The first of these we have already indicated in describing our attempt to get Polly up to the same age as Norma, hoping that when this happened, the two personalities would coalesce. In this we failed, as already stated.

Our second procedure was to keep her in the Norma stage as much as possible. This was done by hypnotizing Polly and waking her up as Norma.

Whether this method would ever have succeeded alone we cannot say. It was slow and uncertain for two reasons, the Norma personality brought back in this way never seemed to be really permanent. At first, Norma would only keep awake a few minutes, but gradually this time was lengthened until, on some occasions, it was a half-day. The greatest difficulty was encountered in Polly's objection to the procedure. Her objection was purely a childish one, that she did not want to go to sleep; she was ''afraid she would miss something'' and we always had to talk, and persuade, and cajole, to get her to consent to be quiet and go to sleep at our suggestion. She was always a good subject and whenever she would get quiet it was only a few minutes until she was in hypnosis and then could be very promptly awakened up as Norma.

The going to sleep of Polly was always on the ground of health and never on the ground of being transformed into

Norma. This was because Polly did not understand Norma and, of course, could not appreciate the situation.

The third plan, of restoring the memory of one personality for the other, was suggested accidentally one day when I was trying to persuade Polly to let me put her to sleep. She was unusually argumentative. She insisted that she did not need to go to sleep because she was not sick. I then tried to explain to her about Norma, that when she was well she was Norma and I asked her if she did not want to be Norma. Her reply was, "I do not know. I do not know what she is like." Whereupon, I replied, "You let me put you to sleep, and I will introduce you to her." She laughed and said, "Introduce me to myself." I said, "Yes, come on." The novelty of this seemed to appeal to her and she submitted. She was quickly in hypnotic sleep and I said to her, "Now, you know Norma, just what she is like and all about her, and when you awake as Polly, you will remember." I then awoke her and she had full memory of her life as Norma. I then put her to sleep again, telling her this time (in hypnosis) that she would awake as Norma and would remember Polly. This also was successful.

This was the turn of affairs. Whereas up to this time we had had with us Polly most of the time and only occasionally Norma, from this time on it was generally Norma and only occasionally Polly. We had, however, quite a long period of struggle to overcome the nerve habits already formed. For some time we had the reappearance of Polly occasionally. We were surprised to find that what we now had was a blend of the Polly-Norma personalities rather than the Norma we had known. She called herself Norma and behaved in the main like Norma, especially in the morning when she was rested. But as she got tired, especially late in the afternoon, the Polly characteristics appeared more or less strongly, although she did not admit that she was Polly. She was irritable and troublesome and argumentative as Polly was, not at all the quiet, well behaved Norma who always accepted our slightest advice or wishes.

Shortly before Christmas we had another surprise in what we took, for a time, to be another personality which we thought might be the true person rather than Norma. One evening when Polly was asleep but somewhat restless and evidently not asleep for the night, as we had learned to understand what that meant, she suddenly spoke without waking up and called the name of the lady with whom she had lived some three years previous.

There were two or three people in the room and Mr. Rausch replied as though he were the lady whose name had been called, saying, "Yes, Norma, what is it?" She said, "Is it time to get up and go to school?" Mr. Rausch replied, "I think it must be." Whereupon, she very slowly wakened, rubbed her eyes, and looked around and then said, "Where am I? This is a hospital, is it not?" She knew none of the people in the room, although they were all familiar to her as Norma. She soon went to sleep but appeared again the next day. This personality also we could bring back by hypnotic suggestion.

It proved, however, to be a case of amnesia, she having no memory of any events of the past three years. When we discovered that it was merely a case of amnesia, we ceased to encourage it, and after a period of perhaps a couple of weeks this personality never appeared again. Gradually the Norma personality became established and Polly rarely appeared. There were, it is true, a number of interesting episodes which we cannot take time to narrate here.

For the past two years there have been practically no lapses and her life has been largely uneventful. She is not strong physically, and not able to do much hard work. It is evident that her nervous system has become rather badly shattered and it may be sometime yet before she is restored to vigorous physical health.

We will now turn our attention to her early history to see if we can find the cause of this condition.

EARLY HISTORY

Norma was born in 1902 (April 8), one of twins—of the type known as identical twins. They were the first born, the mother being twenty and the father twenty-two at the time. The home was good, the mother an attractive woman, a faithful wife and good mother. The father was an accountant earning good wages. There is no abnormality in the family. Children were born until they numbered ten.

The twin sister died at ten of diphtheria, David at eight of tuberculosis, Ophelia at eleven of tuberculosis and Lorenzo at two of spinal meningitis. Norma seems to have had a fairly happy *early* childhood, though always frail; her first grief came at ten when her twin sister died. Norma also had the diphtheria at the same time as her sister. She remembers her sorrow at the death of her sister. They were always together and fond of each other. Their mother could not always tell them apart and Norma relates that sometimes when Eleanor was sick, she (Norma) was given the medicine.

Norma became adolescent at thirteen years, three months. Her mother had not told her about this epoch in her life, and as a consequence Norma was greatly frightened at her first experience. When she was fifteen her father contracted tuberculosis and after a year in a sanatorium died of the disease. With the father's sickness many cares in the home devolved upon Norma and much work helping her mother care for the younger children. She remembers that she always felt tired. She did not sleep well, and had many dreams. But the maximum of trouble was not yet. Not long after the father's death her mother also developed tuberculosis. At one time Norma was ill upstairs with influenza and her mother in bed downstairs with tuberculosis. She was then sixteen. Her mother was taken to a sanatorium and died there a year later. Thus at the age of seventeen she was orphaned.

She had already given evidence that her nervous system—never vigorous—was breaking under the strain. When her mother went to the sanatorium the home was broken up, and the children scattered. This was a severe shock—a tragedy in her life. Norma went to live with a friend of the family. She was then in high school. She would have finished the first year but ill health compelled her to leave school in March. That winter she lived with a distant relative. The house was large and desolate. The woman relative slept on the first floor and Norma was compelled to room alone on the second. Each night she must go upstairs in the dark. Naturally timid and afraid of the dark she could not sleep under these conditions. She would sometimes creep downstairs and stay in the kitchen. Once she was so frightened she awoke the woman and received a scolding. To cure her fear the woman kept teasing her by talking about robbers, *e.g.*, as the child neared the top of the stairs on her way to bed in the dark the woman would call out to an imaginary burglar, "There's your girl, grab her." In the spring she returned to the home of the friends with whom she had first resided. They noticed a marked change in her. Soon after this she had an attack of somnambulism. She had walked in her sleep as early as six years of age. This time she fell over in what seemed to be a fainting spell but which proved to be only profound sleep. She wakened in a few minutes. These spells were rare at this time and had no other symptoms.

In August of that year, 1919, she went to the city to earn her living. In September her mother died. Soon after this the sleep walking became a habit and once she was found with a carving knife in her hand. She explains that she was dreaming of snakes and in order to protect herself she got the knife to kill the snakes. In order to safeguard her it was thought necessary to lock her in her room at night. This was an added cause of worry and suffering, which hastened her breakdown.

Her health was evidently getting steadily worse and one morning while at her work she fell off her chair in deep sleep and somnambulism. In this state she was so violent that she was locked in her room. The next day she was taken to the city hospital. She slept for two days and could not be wakened and later had repeated periods of sleep during which it was impossible to waken her. After seven weeks she was sufficiently recovered to leave the hospital. Though no longer ill enough to remain in the hospital she was not strong enough to work. She was accordingly placed in a home for *wayward* girls. She remained there for three and a half months. This was perhaps the most unfortunate experience of her life. Finally on the 15th of August, 1920, she secured a position in a good family where she took care of a three-year-old boy. She was fond of the boy and enjoyed the work, but in her condition the work was evidently a severe strain on her constitution. In November she had a short vacation when she visited in her home town. At this time she had another attack of sleep walking. August, 1921, she was again sent on a vacation to the family who had adopted her youngest sister, now four years old. This was in her home town and she enjoyed herself visiting her girl friends. Her visit of two weeks was to end on Sunday and she would return to the care of the little boy. On Thursday previous to the Sunday she suddenly fell into somnambulism and after a troubled sleep with violent struggles awakened as the four-year-old Polly already described. She could never be left alone. When awake she was liable to fall at any minute and when asleep would throw herself out of bed if not watched. When held in bed to prevent her falling to the floor she became violent and struggled with super-normal strength for some minutes until exhausted, when she would fall back limp and relaxed, sound asleep. This might be repeated several times or she might wake up in either personality. At this time the secondary personality was always the four-year-old Polly. As usual in such cases the one personality remembered nothing of the other. Nor is either personality able to recall what she is dreaming of when she struggles so violently in her sleep. Each personality takes up the thread of life where it left off. On the morning of Sunday, September 10, Norma was eating candy. She suddenly fell asleep and awoke as Polly. She was Polly, when awake, for six days. The following Saturday at noon Norma came back. Her first words were "Where is my candy?"

At the end of the three weeks above referred to she was sent by the judge of her county to the Bureau of Juvenile Research for observation, study and treatment. This brings us to the point where we started September 22, 1921.

This case shows the usual phenomena of hysteria, some of which have been related: the anæsthesia, with the different forms; the amnesia, possibly also a mild case of neuralgia in the muscles of the neck which was of very short duration, perhaps a couple of days, may have been genuine. A more persistent case of indigestion also seems more likely to have been a real disturbance. The peculiar thing here is not that these things occurred but that there were not more of them during such a long period. This may possibly be accounted for by the youth of the patient and consequent poverty of ideas and perhaps also to the fact that we were also extremely careful not to suggest things to her. We never asked if she had this or that symptom. We never allowed her to think that we paid any attention to anything that she spoke of as her own symptoms and we never discussed her case in her presence except when we were making definite suggestions looking toward her recovery.

We even went so far as to refrain from discussing it when she was asleep. It may have been a superstition but several of us had the distinct feeling that although when she was asleep, nothing could awaken her, yet somehow what was said registered in her nervous system and was acted on later. Whatever may be the cause, the fact remains that minor hysterical phenomena were rather few.

In addition to these, there were other manifestations, such as one expects to find in a psychogenic case. For example, we had a marked case of transference, which finally ended as abruptly as it began. The vita sexualis was manifested through a hallucinosis incestus patris. This was somewhat unusual in its persistence and in the fact that it was concurred in by all the personalities in the hypnosis. She had told this in the home where she was staying before she came to us and it was reported to me by them as one of her unfortunate experiences. She told it to me on the second day and was evidently living through the imaginary experience in the restless sleep which we have described. There were many things to make one believe that it was a genuine experience and not an hallucination. But the fact that we were dealing with a phase of hysteria where, as some one has put it, when we do not find this particular hallucination it is the exception rather than the rule, this coupled with her statement that this occurred when she was fourteen years old, but had never been mentioned to any one until she was nineteen and a half, compelled me to hold fast to the hallucination possibility, even in the face of the constant reiteration by all the personalities, as already stated.

It was of course important to get at the truth in this case because if this were not an hallucination, it could be taken as an important element in causing the trouble, whereas if it were an hallucination, it was a result and not a cause. I was led to adopt the procedure which, I think, finally elicited the facts.

First, the incompleteness of her acting during her sleep; as stated, it was pretty clear that she was acting out this episode. At first, I was fearful that other observers would discover the truth, but soon it became evident that the acting and the talking, for she did both, were never definite enough to give a person who did not know of the supposed episode, any clue. I next found that the vagueness and indefiniteness that she used in talking about it were not due, as I had first assumed, so much to the delicacy of the subject as to her own defective picture. As soon as I realized this I began a systematic investigation of what was in her mind in connection with the matter. It was necessary, of course, to carefully avoid leading questions and also to avoid giving her an opportunity to develop her hallucination, if it was such, and also to avoid the appearance of making much of the matter. I hit upon the plan of asking her one question at a time, days apart, until I was finally satisfied that I had gotten from her the complete story as it existed in her mind.

The story thus obtained was so clearly the imagining of one totally ignorant even of the human anatomy that any idea that it could have been a real experience was absurd. When I later explained this to her, putting it on the basis of a dream, she admitted that it might be.

The next problem was, why did a young woman without experience and with an unusually pure mind in regard to such matters, have such a dream or imagination. The answer seems to be found in an unfortunate environment to which she was exposed for a period of two or three months not very long before her final breakdown in health. Indeed, she had had a partial breakdown and it had been necessary for her to be taken to a hospital where she had had curious spells of sleeping two or three days at a time without waking. She finally recovered from this and left the hospital although not in a physical condition suitable for her resuming her work. While in this condition she was cared for by a semi-public charity, which deals mainly with girls who have fallen morally, but who are being helped back to a normal life. In this environment her attention was drawn to these matters and her claims not to have had such experiences as the rest were more or less ridiculed by the other girls. Vague references were continually made both by the girls and by their caretakers. No real information about the truth of such matters was given. This seems to be a perfect way to develop fears and fancies, to arouse abnormal cravings with no real knowledge or explanation to keep the balance. In her weakened state physically, it is probably only natural that she should dream that something had happened to her.

After about three months, she again secured a position, although the events certainly proved that she was in no condition physically to earn her own living. All this has a bearing upon our final problem of the origin of the dissociation that resulted in the dual personality.

One could hardly expect to find more perfect conditions for the development of a dissociation or a better illustration of how it is brought about. The theory of dissociation is clear and well established. We recognize that many of us, perhaps every one who has an active brain or who lives more than a humdrum existence accepting whatever comes without question, has periods of day-dreaming in which he lives in a realm quite different from that of his everyday life.

There are two types of these dreams, the purposeful and the purposeless. Perhaps they might be called the normal and the abnormal, or the useful and the harmful. The inventor, the creator of new conditions, first dreams them out, lives through them in imagination and then makes them real or seeks to do so. If the dreamer be a poet, he embodies his dream in a poem that enriches human life. If he is an artist, he may put it on canvas. If he is an engineer, he will embody his dream in a new structure of some sort. If he is a novelist he gives us a new story. If he is an inventor, he embodies his dream in a new device that contributes to society's comfort and welfare. If, however, he belongs to the other group he never seeks to objectify his dreams but is content to escape for a time from the hardships of real life to his castles in the air which he has no thought of ever attempting to realize. Perhaps even these may not be harmful, but may be useful if not carried too far. If they serve merely as a period of rest from the real life which is too strenuous they may, like an hour of sleep, enable the individual to go back to his work feeling somewhat refreshed.

But of whatever type, these day-dreams constitute, in a sense, a secondary personality. The individual is for the time being not himself, but is living in a new world of which he is the king. The difference between this type of duality and that of our Norma-Polly and similar cases that are technically accepted as dual or multiple personalities, lies in what may be termed the loss of identity or the completeness of the dissociation, the amnesia of one personality for the other. The problem of Polly-Norma is the difficulty of picturing to ourselves how these two personalities may become so thoroughly separated that one has no memory or knowledge of the other. In other words, we dream by day as well as by night, but we know that we are dreaming, or have dreamed. How does it happen that a person can live for six days in a character utterly different from her usual character and know nothing about her normal life or when she finally comes back to the normal existence can be totally oblivious to the six days in which she was in a "secondary personality."

If we turn to the neurological aspect of the case, we find that it is not difficult to assume that our regular life is a function of definite sets of neurons, highly elaborate and interwoven but nevertheless carefully systematized. It is not hard to imagine other neurons interlacing with these perhaps but which have their own connections and represent a systematized group underlying a line of thought and action utterly different from the usual everyday life and connected with the other neurons by only a few simple pathways. But to comprehend how these two can be utterly broken apart has been impossible for most people, difficult for every one, and seldom made clear.

The study of Norma-Polly for a period of nearly five years has made it possible to offer some suggestion along this line. We have seen from Norma's history that she has always been a child of what we may call "low vitality." Not dull nor stupid, not slow, nor lacking in energy, but rather the picture should be of a person apparently normal but with *no surplus of energy* or vitality. She has always had a struggle not only in her physical life, her actual strength, but in her psychic life. If she has had some of the joy and happiness, she has also had what many would consider more than her share of sorrow and sadness. Death has taken brothers and sisters, father and mother, from a home which was not one of luxury to be sure, but nevertheless was home. She was plunged into dependency among strangers. From happy associations with her family and her childhood friends, she is driven to a strange city, to seek employment among strangers in the new environment. Here she becomes ill, is taken to the city hospital and later cared for in a public "home."

What else could she do than flee to the world of ideal conditions? Her youngest sister, then four years of age, had been adopted into an ideal home where she was cared for and loved and favored, where she had everything that a child could want. Norma visited in this home. Indeed, was visiting there at the time that her duality began. She had doubtless many times wished that she were in the place of her little sister, dreamed by day and by night that she was in that position.

She had another sister whose name was Pauline, called familiarly, Polly. Apparently, our secondary personality was a combination suggested by these two, so that we have Polly, four years old.

But, again, we are facing the problem of how does Norma lose her identity as Norma and become Polly, four years old. It seems reasonably clear that the weakness which we have mentioned is the key to the situation. If we turn our attention to the neurological aspect once more, we know that the brain neurons are storehouses of energy, that a minute stimulus such as the finest particles of matter that impinge upon the nerves in the mucous membrane of the nose may start an immense amount of nerve action. We may even have what is called by Cajal, avalanche conduction. We may picture the whole brain active and alert. Under such conditions there would be no possibility of one elaborate set of neurons being set into activity without the energy flowing into the other set also and so establishing the identity of the individual in his two phases.

But now suppose that these cell bodies which are the supposed storehouses of energy are, as a result of physical disease or weakness, possessed not of their full quota of energy, but barely enough, let us say, to start the next neuron into activity. Not enough to flood it but barely enough to set it going. Moreover, it in its turn can barely stimulate the next one. And wherever the axon of one nerve cell is in contact with two or three dendrites, instead of having sufficient energy to arouse them all at once, it will arouse only one and that will be the one where the synaptic resistance is least.

Now let us say that something has aroused the Polly consciousness. The neurokyme flows through this neuron pattern with just enough energy to put it into activity. But whenever a neuron is reached that is in contact with the neuron patterns underlying the Norma life, the energy flows not in that direction but into another phase of the Polly consciousness, because the synaptic resistance is lower there than it is in the other direction. We thus have the condition for the complete isolation of the Polly consciousness from that of Norma. Thus she lingers in the Polly state until something happens to arouse the Norma consciousness, or rather until there is not enough energy to even keep a Polly consciousness going, under which condition she sleeps, then she may awaken in the Norma condition and because the nervous energy cannot flow into the neurons that spell the Polly life, there is no knowledge of that life, no consciousness of it.

It is significant that on the one occasion when she passed from Polly to Norma without going to sleep she had all her memories of Polly. In other words, when the energy was able to pass over from the Polly group to the Norma, a complete identity was established. The same thing was later brought about through hypnosis and we gave her the suggestion that she would remember Polly and thus established in the sleep condition a connection between the two which was maintained after she woke.

Moreover, it is perhaps not without significance that Norma's abnormality of consciousness began long before the duality appeared. She even remembers walking in her sleep when she was six years old but her first real difficulty was when she went to the hospital and slept for three days. We might explain this by assuming that there was not enough energy to keep the entire brain in activity and develop consciousness, thereby producing a waking state. Since at that period the day-dreaming had not begun, no one portion of the brain had been trained to activity or used to the exclusion of the rest as in the day-dream of the four-year-old Polly. Therefore, the energy was diffused through the entire brain and being of little intensity produced no consciousness whatever, hence she slept.

If this view should prove correct, it would necessitate the conclusion that the dual personality was the only alternative for long periods of sleep. In either case, the problem would be, according to this view, one of reënergizing the neurons. This was, indeed, the theory that we worked on throughout the entire period of the Polly-Norma condition. There were many indications, many more than we can mention here, that we were dealing with a condition of nerve exhaustion and that recovery would take place whenever the energy could be built up to the proper point. Indeed, the later history is in accord with this view. She has been built up to the point where she can now maintain her normal consciousness but not yet to the point where she can assume great responsibility and face difficult and puzzling situations without a nervous disturbance. The dissociation seems to be overcome and replaced by a complete synthesis. Norma is working regularly a half day and seems reasonably happy and normal in her reactions to her environment.

RÉSUMÉ

The Polly-Norma case seems to be valuable because of its suggestiveness. One case proves nothing. But it should be useful in indicating what we are to look for in other cases. We have not enumerated all the experiences of Polly and they do not seem necessary for the present purpose. These may be published later for the benefit of those who wish to go into the details.

For the present purpose, what Polly did is not so important as the fact that there was a Polly. We have accordingly confined ourselves to that problem and the question of "How did the duality originate and how was normality restored?"

So far as this case is concerned, it would appear that there are two factors in dual personality. First, a divided mental life and second, a nervous system deficient in energy. Without these two factors, we might have had an abnormal manifestation, but hardly a duality. Had Norma's life not been of such a character as to drive her to attempt to escape from reality by imagining a happier and freer existence, there would not have been the two phases to be dissociated. On the other hand had she been in vigorous mental health with a nervous system full of energy, she might have gone on, dreaming of a happier existence but never losing contact with real life.

There is probably more or less of a casual connection between the two factors, for a vigorous nervous system would have enabled her to face the actual conditions with more courage, with the result that she would have more effectively controlled them and there would not have been the necessity for leading the imaginary life.

It is worthy of note, also, that this phenomenon has taken place in a person of unusually good intelligence. This also is probably not accidental. Prince's case of Miss Beauchamp was likewise of high intelligence. This would seem to be essential, that there might be enough imagination to create the ideal world in which the person of secondary personality lives. I cannot conceive, for example, of a feebleminded person ever developing a dual personality.

Our attempted theory of how the dissociation actually takes place is highly speculative, nevertheless neurology and brain physiology seem to be developing in a direction that would render such a hypothesis not impossible.

GLOSSARY

Amnesia, loss of memory.

Anæsthetic, insensitive to touch, pain and temperature; in general, lack of sensitivity.

Automatic Writing, expressing ideas through writing while in a trance, or abstracted state.

Axon, main discharging fiber of a neuron.

Dendrites, receiving fibers of a neuron.

Hallucinosis Incestus Patris, hallucinatory or imaginal sexual relations with the father.

Hysteria, mental disorder not traceable to a definite organic condition, and characterized by a variety of symptoms such as amnesia, anæsthesia, hypersensitivity, blindness, deafness and muscular paralysis. None of these conditions is "genuine"; removable through suggestive therapeutics.

Identical Twins	twins resulting from a single fertilized egg or ovum; it happens that such twins are always of the same sex.
Motor	pertaining to muscular movement.
Neurokyme	nerve energy.
Neurological	pertaining to the nervous system.
Neurons	anatomical units of the nervous system; nerve cell.
Psychogenic	of psychic or mental origin.
Sensation	relative simple experience referred to the functioning of sense organs; experiences such as seeing color, hearing tones, feeling muscular movements and the like.
Synaptic Resistance	supposed resistance of a synapse to the nerve impulse; a synapse is the juncture between two neurons.
Vita Sexualis	sexual life.

READING TEN[1]

A CASE OF *DEMENTIA PRÆCOX:* AN ILLUSTRATION OF EIGHT ORGANISMIC LAWS[2]

Editor's Note

The following case of abnormal behavior, like so many cases of maladjustment, grew out of prolonged emotional stress. It is related here for the purpose of illustrating how emotional disturbances, terminating in insanity, follow the same laws that operate in normal life, namely, the organismic laws that were presented in Reading One.

Dementia præcox is a form of insanity which derives its name, first, from the fact that the disturbance generally commences early—precociously—in the life of the individual, and second, from the fact that its symptoms are those of mental deterioration from an originally normal condition. It is not known to what extent this form of insanity depends upon inheritance, or upon acquired physiological deficiencies, and to what extent it arises from an unhealthful social environment. No definite organic causes can be assigned to the disease in its early stages, but in its extreme and advanced stages a deterioration of the brain is evident. The abnormality is characterized at first by an extreme sensitiveness and an inclination to be suspicious of others, a disposition toward sulkiness and violent temper, a gradual loss of sympathy and co-operation, until the patient shows little or no affection toward his immediate associates, and suspects them of evil intentions toward him. Finally, apathy, indifference and carelessness become extreme, often to the extent of an unwillingness to eat, a refusal to talk, and a neglect of bodily needs. Meanwhile suspicion has developed into definite delusions of persecution which are frequently balanced by ideas of grandeur and superiority, hence the classification, *paranoid præcox.* The only recourse is to segregate the patient in an institution when his condition increases to the state that he will not co-operate with his fellows and cannot hold a wage-earning position in society, and when, as he frequently does, he shows a dangerous hostility toward his associates.

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[1] Parallels *The Science of Psychology,* Chapter VII.

[2] Written specially for *Readings in Psychology,* by Raymond Holder Wheeler, University of Kansas.

J is a young man twenty years of age. As far as can be ascertained there has been no insanity in the family, at least for several generations. During the first few years of his life he was normal and happy. He was an only child and had the advantages and comforts of a cultured home. His father was a well educated, professional man; the mother a sensible, happy, well adjusted woman devoted to the home. When J was four years old the family moved to a small, secluded mining town. Here trouble began for the boy. The community was for the most part rough and uncultured. Children in the neighborhood looked with suspicion and ridicule upon the newcomer, who was dressed better than they, talked almost a different language, hesitated to engage in their types of activities, and probably showed that he felt himself superior to them. It was not long before they ganged against him and began to haze him. Not only did they call him "sissy," "baby," "sweetie" and numerous other derogatory names, but they were continually playing pranks upon him. As a consequence, when he was away from home he was constantly in misery; his normal desire for companionship was not satisfied; he lived in terror of other children, and tried every way to avoid them. Needless to say he hated school. To show brilliance in class was to invite jeers and torment at recess and after hours.

His mother naturally defended him at home, a procedure on the one hand necessary for the happiness and continued normal adjustment of the boy to life, but at the same time a cause for a widened breach between him and his young associates, for it made him more conscious of the difference between him and the other children, and encouraged him to rely upon her. It was during this period that the most genuine efforts in the world to make life tolerable for the boy were the first step in a permanent maladjustment. The mother fought his battles for him and as a consequence the boy became more and more dependent upon her and began to regard her as the only person who was not hostile to him. As his dependence upon his mother grew, his suspicion of others increased. The instant he showed more suspicion of others, even of potential friends, the more they showed by their attitudes and bearing, that they considered him a peculiar child.

Personalities affect each other in devious and subtle ways.

A facial expression, a tone of voice, a bodily pose, a well intended question, a casual remark, all reflect attitudes and feelings that are in the background. None of these need be analyzed at the time, for one personality as a whole adjusts itself to another as a whole, or in other words, responds to the expressions, quality of voice, remarks and attitudes of others as total stimulus-patterns.

The more J avoided his companions and showed his discomfort the more their enjoyment increased and the bolder their hazing became. One day the gang seized him, lifted him to the roof of a shack and rolled him off. Another day they took off some of his clothing, flaunted it in front of him and defied him to catch them and regain his possessions.

A tense situation of this sort could not last. The family moved into a different neighborhood and sent the boy to another school, but this only mended matters temporarily. The boy had already developed traits of unusual shyness and sensitiveness and was still suspicious of his playmates. He would respond at home only to caresses and babying. Efforts to encourage independence and initiative brought forth the suspicion that even a friendly teacher, and his mother, were unsympathetic. But, as will soon appear, the teacher and the mother should have persisted. The combined efforts of friends, however, succeeded in getting J through the grades with evidence that he was forgetting to some extent the painful years of his earlier boyhood. He had become interested in his studies and was making excellent grades.

At the time J was ready for high school, the family moved to a larger town. He was still exceptionally sensitive; his feelings were continually being hurt; he was dreamy, made friends with difficulty and liked but few of his associates. At the onset of puberty and of the adolescent period that followed, his shyness increased; he was awkward and different from the other boys; he became extremely self-conscious and "girl-shy." If he was called upon to recite he became terrified; he was sure that every one in the class was eagerly waiting for him to make a mistake or commit an awkward blunder; his own voice sounded hollow and unfriendly, and of course this caused him to speak hesitatingly and actually interfered with his thinking. His only goal became that of obtaining excellent grades; he spent most of his time in study. He could study and read *by himself,* and when he was alone he was tolerably happy.

But the vicious circle continued. He was queer and knew it; others knew it and showed they knew it, oftentimes unintentionally; but the result was the same; the breach widened. Society was uncongenial, hostile, cruel; there was no relief from tension by participation in normal, social activity; on the contrary, there was only increased distress. Only one avenue remained open for him; he could not seclude himself physically from society; he withdrew mentally. He was brilliant, and would spend hours at a time inventing mathematical problems and stunts, many of them freaks and curiosities, like arranging blocks of numbers whose columns would add up to the same total in both the horizontal and vertical directions. He worked out short-cut procedures in multiplication and division and devised ways of ascertaining instantly the day of the week when a person was born, with only the month and year given to start with. He lived in a world of imagination.

He withdrew emotionally, also. He showed no interest in other people, did not care to make friends, exhibited no affection for any one save his mother. With this withdrawal he lost all social insight. He was unable to recognize a friendly social group when he was in one; he was constantly looking for trouble and suspected friends of talking about him, criticizing him, plotting against him. His personality was splitting, in that the constant presence of a social environment compelled him to react to a real world of objects and people. Where unpleasant social relations were not the outstanding problems to be met, he reacted normally. These modes of behavior expressed one phase of his personality, but to the extent that social relations were involved he reacted abnormally and revealed the symptoms of an organized, mental seclusion. In this case the result of the dissociation is not a secondary personality that responds normally to a social environment, as in the case of hysteria; it is a withdrawal from reality, a shutting off from others.

As a consequence of this emotional withdrawal J lost interest in his studies during his senior year in the high school, and during the last semester he received very poor grades. Then J's parents made a decision that to them was reasonable and sound. He needed to go away from home where he would be forced to rely upon himself; he was brilliant, and therefore the best chance of giving him an absorbing interest in life was to send him to college.

But the vicious circle still persisted. He was quiet, awkward, unco-operative, sensitive, suspicious and indifferent, hence the other boys who roomed in the same house branded him at once as a freak. The ghastly days of his early boyhood were now to be repeated. He saw the other boys whispering and laughing at him, and soon thought that *whenever they were talking together they must be talking about him.* They played tricks on him and teased him. He was an easy target for fun, because they soon discovered that he was utterly lacking in a sense of humor, and proceeded to exploit his condition much to their hilarious enjoyment.

One of a group said to him: "J, you have colored blood in you."

"I have not," replied J, "I have no more colored blood in me than you have."

Because they understood the joke in advance, the other boys burst into peals of laughter, which of course distressed and angered J. Then one of the group turned to the others and said, "He has colored blood in him, hasn't he? His blood is *red.*"

"Yes, yes," they all joined in, as the laughter turned into raucous shouts. Then the group walked off, leaving J puzzled, and burning with bitter hatred. Here is an excellent example of the loss of insight that attends an emotional withdrawal. J could not understand the point of the joke; he could see no connection between "red" and "colored" because he was so preoccupied with the idea that they were "accusing" him of possessing Negro blood.

J immediately went to the head of the physiology department and requested a blood test. When the professor insisted that his blood was colored—it was red—and that it was not necessary to test it because he had no Negro blood, J wondered if the professor did not suspect him of being a part-Negro and was refusing to test his blood for fear he would have to divulge the truth. Upon J's insistence, however, the professor granted his wish and assured him that he was one hundred per cent. "white." A few days after that J asked a friend if it would not be wise to put a notice in the college daily to the effect that a blood test had proved his race beyond a doubt. The friend advised him not to!

Hostility between J and the boys who were thrown in contact with him, soon developed to the danger point. J showed fear and hatred in his facial expression and in his attitudes; the other boys developed a concern over their safety; mutual suspicion fanned the flames, until one day, J "threatened to shoot" one of his housemates. It is not known what J actually said. In telling the story to a friend, J insisted that he was only expressing the idea that "they" ought to be shot for keeping derogatory stories about him circulating over the campus. The boys insisted that he said, "Some day I am going to shoot you." Immediately after this episode J's room was searched by University authorities, during his absence, and a loaded revolver was found hidden behind some books on the shelf of his closet. It was at this juncture that J's mental condition was first brought to the attention of a physician. He was taken to a hospital and placed under observation. A friend, M, saw and talked with him there, for the first time.

When M entered the room J was in bed but rose suddenly to a sitting position, with a terrified and haunted expression on his face. "Hello," he exclaimed suddenly. "What are *you* going to do with me?"

M replied, "J, I am not going to do anything with you; I am just going to talk to you. You have never seen me before, but I am a friend of yours; I have learned that you are in a run-down condition from over-work. You will take some advice from a friend, won't you?"

"Well, yes, if you are a friend," answered J, but his tone of voice showed plainly that he was still suspicious and doubted M's word.

"I have come to take you to an excellent doctor," continued M. "He will understand your present condition, and I am sure he can help you."

Considerable persuading was required before J consented to go with M. But on the way to interview the doctor, who lived about fifty miles distant, J became quite friendly and told M the story of his life. This story was subsequently checked from other members of the family. During this conversation which lasted over an hour and a half, J revealed evidences of mental deterioration, especially if he had started to relate some experience that was unpleasant. Once M asked a question pertaining to a rather personal matter. J seemingly did not understand the question and it was evident that his reply was not an avoidance of the issue. Laughing pointlessly, he almost shouted, "You know I have ridden on every kind of conveyance there is." M tried to follow up this cue and urged J to tell about some of his experiences while riding different "conveyances" but J seemed confused and could give no details about his supposed adventures. Four or five different times during the conversation J would break in with remarks that were utterly irrelevant to the subjects being discussed. Then before the conversation could be continued, it was necessary to start on a new subject. After a momentary period of dissociation J seemed unable to continue the thread of conversation where it had been left off the moment before.

The psychiatrist advised J to leave school, and his mother was asked to come and take him home. While waiting for his mother to arrive, J was kept in the hospital. M visited him every day. Usually he found J standing in the doorway of his room, at the head of the stairs, watching the stairway.

"Why are you standing there?" M would ask.

"Why, I suppose they are coming after me," was a typical reply.

"Who is coming after you?"

"The police, to arrest me."

"J, you haven't done anything to warrant your arrest. The police are not coming."

"Well, they are trying to get me, anyway."

At another time M entered the room and found J on the bed, reading a magazine. He jumped to his feet and asked suddenly, "What is the doctor going to do with me? There is nothing wrong with me. Why don't they let me go back to school?" M tried to explain to him that he had been studying too hard and was tired out; that he wasn't doing well in his studies and needed a rest. (This last statement was true. J was in danger of flunking out of college.) When J's mother arrived, J showed a genuine affection for her and was very solicitous of her welfare. The next day they departed by train. M took them to the train and J's last words, as the train moved away from the station, were, "Thank you for what you have done for me; you are a real friend." M received two postcards from J, mailed *en route* to the latter's home. Neither one was coherent!

J was gradually developing a genuine case of *paranoid dementia præcox,* but had probably not advanced so far that a congenial environment and constant efforts on the part of an intelligent attendant might not effect a cure. Several months would be required to convince J that the world was not hostile toward him and that he had a chance to live an unmolested, normal existence.

This case shows that abnormal behavior demonstrates each of the eight organismic laws that were given in Reading One. The *first* law, the *law of field properties,* is illustrated in the same way, of course, that the behavior of any individual illustrates it. J was a member of society and participated in the folkways and traditions of the group in many respects just as a normal individual would.

The *second* law, that parts derive their properties from the whole, is brought out with unmistakable clearness. With the exception of the home, the social group was hostile; J became hostile. J's concept of himself was one which the social group had given him by their attitudes and remarks. He was peculiar, different, a freak. The attitude of his home environment both helped and hindered him in developing a balanced conception of himself. It helped to offset the sting of unsympathetic attitudes from without and permitted him to acquire some degree of self-respect, but because of its lack of persistence it also prevented him from assuming an aggressive attitude toward his companions and delayed the time when he should have developed initiative. Accordingly, he acquired hostility from outside the family and sympathy, co-operation and friendliness, only, with respect to the limited group within. When in one group he was one person; when in the other, he was a different person, thus illustrating the *law of derived properties.*

Law *three,* that the whole governs the activities of its parts, a corollary of law two, has already been demonstrated by implication. Had some outstanding personality, with influence, prestige and accomplishment, been known to J in his earlier years, the catastrophe of his breakdown might have been avoided. The leader and a group are not two separate agencies which influence a person in different ways. They are a unit, a total situation, for a leader is a person whom the group *accepts;* his leadership hinges upon his relation to the group and its attitude toward him. In responding to a leader, one also responds to the social situation in which the leader is found. One senses the respect of the group for him, and learns of a life-situation that is healthful, agreeable and profitable. Hence, J might have made the proper adjustments through the processes of imitation, sympathy, and suggestion that he would have observed around him had some one of influence taken an interest in him. As it was, the leader-group situation was lacking which J needed in order to forestall the development of a distorted personality.

But perhaps the most striking illustration of this law is the deterioration that had commenced in the later years of J's maladjustment. Human nature decays in isolation. J was shutting himself off from his social environment, refusing to recognize it; he was socially anæsthetizing himself; he was therefore losing ability to understand others, losing initiative, ambition, and purpose, because these traits depend upon social situations. In cutting himself off he was depriving himself of a personality. The more extreme cases may ultimately lose all modesty; they may refuse to talk, eat, wash, or even to use a toilet. They will remain where they are placed, for hours. Raise their arm and they will let it remain there almost indefinitely; put a hat on them and they will keep it there whether indoors or not. It is evident, therefore, that in the absence of the social group (the group is essentially absent if it has no effect on the individual), personality degenerates. With nothing upon which to feed, no whole to maintain the properties of the parts, the parts waste away until there is nothing left. All this illustrates the *law of determined action.*

Fourth, parts emerge from the whole through a process of individuation. At first, J had the attributes of any normal, undeveloped personality. They were all general and undifferentiated, as in any child, but out of an unharmonious and unrelated mixture of qualities in the human nature-pattern around him—only hostility on the outside and only sympathy in the home—he emerged a disorganized personality. He had two social environments which were for him the antithesis of each other; he knew no social setting in which the two were balanced; he was part of the two wholes, and two wholes developed within him.

Fifth is the *law of field genesis.* A divided personality is, after all, a whole whose parts are only relatively independent and segregated. The quiet, reclusive, dreamy, indifferent J was a personality that had to live in a society, had to keep its contacts with the social group through modes of behavior like those of other people. But this side of his personality was growing, as an organic whole, and as this was growing the social J was gradually disappearing. If the process had continued, J would have lost his personality, completely, through the lack of a dynamic relation to the human nature-pattern around him.

Sixth, J's maladjustments occurred in the line of *least action.* His tensions not only demanded resolution but demanded it in the most direct and immediate way. Recall that the presence of a tension implies a remote end, a goal in case of human behavior. The one does not exist without the other. What then, were the goals toward which J was striving, and what was J's behavior in resolving his tensions in the direction of these goals? Space permits a discussion of only a limited number. First, there was his mother whom he sought in order to secure relief from the distress which the neighborhood gang was causing him. But since a part of a situation is always responded-to in its relation to a total situation, not only the boys themselves but the physical environment in which they roamed caused him discomfort. He did not like to play in the street or in the nearby park, and he hated his school and everything about it, even when his tormentors were not around. Constant stress meant a constant seeking of relief in the direction of his mother. It was not long, therefore, before he developed a "mother fixation" which remained with him permanently. His mother was the only person who could do anything with him; his dependence upon her was altogether too great, in fact, complete. For this his mother was not without blame; she permitted herself to become the goal in too many ways, but she did her best with the knowledge at her command. J failed to develop, sufficiently, the qualities of dogged persistence and a determination to conquer his troubles. His mother feared to press him. Because the goal at home supplied a more direct and immediate relief from strain, there was no effort to seek goals outside the home, and no solving of difficulties for himself. *It was the goal at home that prohibited him from having other goals.* There were no "low pressure areas" in his social environment so long as "the *lowest"* was in the home. Under the circumstances, J *could not* develop strength of character; the ways of acquiring a normal personality were deprived him.

Second, drawing into his shell, a process that had been going on gradually for fifteen years, was a goal activity. J's mother could not resolve all of his tensions; she could not be the goal in every situation; but since society furnished him none, what was he to do? The same total situation that arouses tensions supplies the goal. While the hostile social group was inducing distress in J, with the mother's help, it was also setting up the goals toward which those tensions were to be relieved. These were imaginary goals. A normal boy would actually fight if attacked, for his anger would demand resolution in that direction. He would also have plotted revenge and executed the plot overtly. But J did all this in day-dreams. Here was the beginning of a shut-in personality. The circumstances of his life produced a recluse as an inevitable occurrence in the line of least action. The "area of lowest pressure" was within, when it could not, under the circumstances, be outside.

The *seventh* organismic law, that an energy system resists disintegrative influences from without, is illustrated in a striking way in J's *suspicion*, first, of youthful companions who might have proved to be friends, and finally, of every one with whom he came in contact. Suspicion is a form of resistance and depends upon imagination; it is an anticipation of danger, of thwarted purposes and of hostility, and usually expresses itself in fear. Now, to be hurt physically means to suffer a certain amount of physical disintegration, with attending pain; similarly, mental anguish comes with disturbances of one's normal habits and aims, for these disturbances mean at least a temporary disintegration or disorganization of one's behavior. Remember that conscious behavior is the activity of an energy system. Conscious beings, therefore, resist any interference with goal activities for precisely the same reason that any energy system resists being changed. The system is either in a state of balance or it is approaching a balance. J's conception of himself represented a balance of energy within the total organism. Suspicion was an act of resisting the disturbance of this balance which outsiders were continuously inducing. This resistance counteracted the disintegrating influences and functioned to maintain unity within himself, and involved an expenditure of the maximum amount of energy available under the existing conditions. In other words, the behavior of the individual follows the *law of maximum "work."*

A hostile gang prevented J from enjoying a normal outlet for his desire for companionship with children of his own age. This meant a serious disturbance of organization in his social behavior. Normal resistance, as we have seen, was prevented inadvertently by his mother and also by his day-dreams. But the same imagination that was an instrument in resolving his tensions through the day-dream, was the instrument of resistance. He resisted trouble that he anticipated; he suspected evil of others and showed it in his attitudes of fear, unfriendliness, shyness and hostility. For him these were costly ways of trying to maintain the unity of his personality, and of trying to preserve self-respect; but he had no other recourse.

Finally, under the *law of configuration,* falls a discussion of J's insight. J's insight at the outset was evidently as normal as that of any child. But it became restricted and distorted, because of a social environment that, compared with the average environment of the normal child, was restricted in the outlets it afforded, and unbalanced in the types of human relationships which it furnished. In accordance with the principle that details of a total situation are always perceived in their relation, it was only natural that J should have developed a very incomplete and unfortunate understanding of human nature, for the relations perceived were limited and undesirable.

Before J had reached the age of adolescence, persistent and well guarded advances on the part of any companion would have been required to furnish him with the insight he needed in order to change his attitude toward society. Such a companion would have met with a strong resistance. In order to break down this resistance an unusual interest in J would have been necessary, more interest by far than the ordinary fifteen-year-old boy or girl would take in a person of their own age who was queer, awkward, and therefore distasteful. Naturally, then, he was avoided, and the more he was avoided the less insight he acquired in proportion to the degree of his responsibilities. The demands of a social environment mounted far more rapidly than his social intelligence could grow in the absence of *rapport* between him and the group.

But this is not the whole story of J's insight. The most tragic aspect of it all was the fact that J was bound to maturate. If insight cannot grow normally it will grow abnormally. It did so in J's case. His distorted view of society grew more distorted; his limited insight became more limited. His shut-in personality became more shut in. His suspicion of others became stronger and stronger until finally he was having delusions of persecution. His fear that the police were coming to get him was only a natural product of maturation. It needed no explicit suggestion.

His case will undoubtedly be hopeless unless some one can check this maturation through re-education and companionship over a long period of time. He cannot do it himself for he cannot lift himself by his bootstraps. To be intelligent and to be normal, one must live in a normal social environment. J might have developed normally in spite of his unfortunate social experiences if there had been other children in the home whose behavior had been normal, and who could have imparted to him the qualities of human nature necessary for a balanced personality. Insanity, like a tumor, will grow in the individual unless it is checked in its incipient stages, or unless its deepest roots are cut out. Maladjustments will grow unless their causes, whether social or otherwise, are removed from dynamic contact with the individual.

It is quite possible that J was nervously unstable at the outset. His physical constitution was not of the strongest. But whatever the predisposing cause that might have existed, it seems fair to assume in his case, that *dementia præcox* was socially conditioned. There may have been deficiencies in the chemistry of his digestion, in his blood or in his nervous system that facilitated the onset and development of his mental aberration, but it is reasonable to suppose that a healthful social environment might have prevented such a physical condition, if there were any, from leading to insanity.

READING ELEVEN[1]

A CASE OF RETARDED DEVELOPMENT IN THE BLIND[2]

Editor's Note

Dr. Cutsforth has described a most striking case of retardation in the blind that, like the *dementia præcox* case in the preceding reading, is a *social* product. The social environment in which Bert lived for twenty-four years permitted little intellectual as well as emotional growth. His condition at the end of this time well illustrates the fact that abnormal behavior follows organismic laws, and reveals the tremendous rôle that emotion—and the lack of it—plays in the development of a personality.

After the death of Bert's mother, it was possible for his brother to insist that Bert be sent to school. Although twenty-four years old, Bert had never been inside a school room, because he had been partially blind from birth from the effects of congenital cataracts. It is very difficult for most parents to send a defective child to a special institution, although it is most advantageous for the child. Bert's parents not only objected, as most parents do, but absolutely refused to commit him to the care of others. The compulsory school laws of the state in which they lived were very lax, and Bert was permitted to grow up to his twenty-fifth year in a small family circle consisting of his father, mother and a brother ten years his senior.

It was recognized from the first day that Bert entered the state school for the adult blind that he was to be a special case. In spite of the fact that he had been partially blind from birth, he was more helpless and less adjusted to his condition than those who had lost their sight only three months previous to entering the institution. Due to the development he had made in the sheltered home, his appearance and behavior were more ape-like than human. He was immediately pronounced hopeless by his teachers, regarded with annoyance by his fellow-students, and thought to be insane by visitors at the institution.

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[1] Parallels *The Science of Psychology*, Chapter IV or Chapter VII.

[2] By Thomas D. Cutsforth, University of Kansas. This case was investigated while the author was a Fellow under the Social Science Research Council. Written specially for *Readings in Psychology*.

Bert was of average height, but forty pounds underweight. The latter condition was due to the many food aversions he was permitted to develop in his home, and to a pointed dental arch and irregular teeth which did not permit proper mastication. These conditions caused chronic gastronomic disorders.

His personal appearance was most objectionable, for he had acquired a double curvature of the spine which caused his shoulders to droop forward, and at the same time, he would sit or stand with his head thrown back so that his face was turned constantly skyward. He had the habit of keeping his mouth wide open and, due to the position of his head, he had developed a tic-like cough or bark, originally caused by saliva draining into the trachea. This tic-like bark occurred in a rhythm of about one in each minute and a half. When either walking about or passively sitting, he continually fluttered his hands before his face or between his knees. His manual activity would vary to the manipulation of some erogenous area of his body.

At the age of twelve, Bert underwent an operation which restored a very small field of relatively clear vision to his right eye. This small field he utilized to the maximum in close vision with the aid of the eye-piece of a microscope. For distant vision, he carried a short telescope which he employed in scrutinizing objects and passers-by.

Since Bert had never been away from home before, with the exception of attending church with his parents on Sundays, his social behavior was far from that of the normal person. At the school he much preferred solitude, and when it was necessary to be in a group, he would withdraw to the most remote corner, where he would busy himself with his usual round of automatisms. If an attempt was made to engage him in a conversation, he would cover his face with both hands and bend double until his head was below his waistline. In this position he would mutter incoherently to himself in a language which was hardly recognizable as English. His enunciation still possessed all the primary defects of the child just beginning to talk. His parents had neglected to correct a single one. If the conversation were persisted in, Bert would either push away the intruder or make a rapid escape. He wore his hat wherever he went, and would trample and jostle his fellows in the halls; and at the table, he possessed not even the most rudimentary knowledge of the table service.

For the greater part, Bert was physically lethargic, although he enjoyed short walks. Toward his objective environment he was decidedly apathetic. He occupied most of his leisure time with subjective activity in the nature of phantasies in terms of auditory and vocal-motor imagery and enjoyment of autonomic perceptions. Due to his ability to see a limited area, he had never developed that tactual curiosity which is so necessary in the education of the blind. If he did become interested in some object, he would satisfy all curiosity by a cursory peer at it through his glass. At first it appeared as if he were interested in nothing outside himself, for, although he had never been in such a place as the school before, nothing attracted his notice or curiosity. He seemed to take everything for granted, with the possible exception of the weather. Meteorological changes were a constant source of interest to him, and it was only by employing the topic of the weather that a first conversation could be held with him by fellow students. He had decided opinions on every phase of the weather, which were governed by some formal verbal rule of thumb such as "Rain always follows an east wind."

Apparently Bert had no social sense which applied to the situation of the school. He was oblivious of every one so long as no situation arose which demanded a response from him. Whenever it became necessary for him to recognize overtly the presence of another person, he would retreat to a more distant position. This negativism would gradually wear off as he became acquainted with any particular individual, when an attitude of indifference would take its place. He spoke voluntarily and unsolicited but twelve times to one person who was with him at least three hours each day for eight weeks. He roomed with three other pupils for six weeks before learning the names of any of them. When accustomed to a person he was obedient in the extreme, and would comply readily with any reasonable request, but the request would not hold when a similar situation occurred. He would remove his hat when entering the building, if requested, but five minutes later he would enter again without a pretense of removing the hat.

Bert possessed a rigid set of morals and ethics in the abstract which applied to people in general but not at all to himself, save in one respect. He was always prompt and on time at classes and at appointments. He could not leave a class room before the allotted class period was over without considerable emotional upset, even though he might have been excused by the teacher. Apparently he was as unaware of social attitudes toward himself as a small child, for the presence of people did not modify his conduct in the least. He would sneeze, cough, and flatulate regardless of his surroundings, and when told that such behavior was not permitted when others were about, he gave no evidence of comprehension or chagrin. The extent to which he was apparently immune from social attitudes is evidenced in the fact that he had constructed no compensatory defenses about his behavior. In a group, he felt bewildered and afraid, like a little child, without knowing why. The one healthy manifestation of a normal self-regarding attitude to be found in his whole range of contacts with people was that he apparently enjoyed a situation in which one with whom he was closely associated was placed in some inferior rôle. In no other sort of situation did he exhibit any sense of humor.

Bert's attempts to learn the manual crafts of the school were as perplexing to the teachers as they were confusing to him. His manual dexterity was approximately *nil.* He had been taught at home to dress himself, more or less, so long as he wore the sort of clothing to which he had been accustomed, but a new garment of any sort presented insuperable difficulties. In his attempts to manipulate materials and tools, his hands were awkward and flaccid. Although his hands were very well shaped, the prehensile function of his thumb was not employed in handling unfamiliar objects. After being taught to grasp the handle of a hammer he learned to drive a nail into a board, but he was never able to manipulate the hammer in withdrawing a partially driven nail.

In all his manual efforts, his restricted vision was of more disadvantage than aid to him. It was so small in extent that he would have been better without any for he could see but a small portion, at a time, of that which he was working upon. With his hands occupied with materials and tools he could not use his special glass. The greater part of his effort was employed in arranging the material in a way that brought it into his field of vision. In this task, considerable ingenuity was shown. Apparently, the attempt to see had been a great factor in producing the ineptitude of his hands.

As preliminary training for the handicrafts, Bert was put to work on the Montessori peg boards. He was first given the peg board in which the pegs are of varying diameters. He was able to complete this one in eight minutes the first time and in four minutes on the second trial. He had much more difficulty with the board which had pegs of the same diameter but varying lengths. His first trial was completed in eighteen minutes and the second trial in ten minutes. It was apparent that Bert could complete a manual task if it involved relationships which were simple, constant and uniform. Another large factor in the mastery of the peg boards was the fact that they were one of the few tasks Bert had attempted which he enjoyed and believed were not too hard for him. His first reaction toward all tasks was to question whether they appeared too hard for him, and he took the attitude that most of them were too hard. The only other manual task given him which he liked was stringing large wooden beads upon a strip of rattan. He liked this task very much and did it well. He did it well either because he liked it, or he liked it because he could bring both the wooden bead and the end of the rattan within his small visual field.

In nine hours he failed to learn to tie a double bow knot, although he had been tying single bow knots in his shoe laces for a period of years. He failed in fifteen hours to learn simple three-strand plaiting. In eight weeks he made but the slightest progress in basket making and chair caning.

During his second week at the school, Bert cautiously inquired of one of the teachers if he could use a piano in the music building. It was arranged that the teacher should take him over to the music building that evening. Since this request was the first evidence of any interest Bert had taken in anything about the institution, the teacher was anxious to see what he would do when motivated by a self-imposed desire. He was seated at the piano in one of the practice rooms, and much to the surprise of the teacher, he struck a full two-manual chord, strongly and full of confidence. Then he arose and said that he did not want to play on that piano as it was out of tune. He was given a second instrument and it was rejected also for the same reason. He was apparently satisfied with the condition of the third, and oblivious of the teacher, he proceeded to play selection after selection. Apparently, when he forgot parts of a selection, or certain parts were too difficult for his technique, he would, without hesitation, complete the part with his own improvising.

By questioning him regarding his musical training, it was discovered that he had been given a few piano lessons several years previous. He had learned quite a number of selections and had voluntarily learned a great many more by ear through his attendance at church. He said it was his habit while at home to play every evening for thirty minutes or an hour. It was evident that the formal selections which he had once learned correctly had suffered alteration from his ability to improvise. He much preferred the selections of his own building to those which had a formal composition. He possessed a good sense of harmony and very rarely struck a discord. When one did appear it was caused from finger manipulation rather than faulty perception.

During his third week at the school, one of his teachers discovered that he possessed considerable knowledge of, and interest in, geography. He was taken into the geography room which was filled with splendid equipment such as raised topographical maps, globes, and dissected maps showing political divisions in separate pieces. Bert took but a cursory peer at each map in turn with his special glass, and was apparently satisfied. He could not be induced to make a tactual examination of any of them. While wandering about the room, Bert discovered an ink-print, geography text upon a desk. He took up the book and opened it to the title page, and remarked that the book was a good one, but it was now out of date since the last edition had been published in 1915. He sat down with the book and his glass and read for the remainder of the class hour.

His geographical knowledge consisted largely of verbal descriptions of cities, rivers and mountains. He knew the capitals and principal cities of all the states in the Union and of the countries of Europe. Although he possessed a rather complete and accurate verbal knowledge of salient geographical information, he had formed practically no relations between the different items of information. He did not know the position of any of the cities or states outside his immediate state. He could give no reason why New York was the largest city in the United States, nor could he venture a guess why there were more cities on the Atlantic Coast than there were on the Pacific. His geographical information seemed to be retained in almost discrete verbal units, in terms of rote, verbal memory.

Subsequent to the removal of the cataract from his right eye in his twelfth year, his mother taught him to read. Apparently he enjoyed reading a great deal, for he not only read daily papers while at home, but also several boys' magazines. He had read the Bible through, and also several other books. He had learned in the last couple of years two systems of Braille point-reading, and read magazines printed in Braille with much less facility than he did the ink-print. He had a rather large reading vocabulary, but most of the words had a very vague meaning for him. He could verbally describe many things and objects which he had read about, but he did not know exactly what he was describing. He described a ski quite minutely, but utterly failed to recognize one when it was presented to him. However, this is not a difficulty peculiar to Bert, for it is common in the mental equipment of the congenital blind. Bert's condition merely represents an exaggerated case of the same deficiency.

After learning to read ink-print, he learned, on his own initiative, to do print handwriting with a pencil. While in attendance at the school, he was able to write his own letters home with the pencil.

From his first experience in the typewriting class, he appeared to like the typewriting lessons. The first day he permitted the teacher to show him how to manipulate the typewriter, but thereafter he would not allow the teacher to show him anything about the use of his hands on the keyboard. He learned the use of the typewriter in fourteen hours' work. His manual form on the typewriter was far from being correct, but he could write, and employed a combination of the visual and tactual methods. Some days he would do the lessons set by the teacher, but more often he would pursue his own whims and improvise copy-sentences for himself.

Previous to his coming to the institution he had always been shaved by his father. This, to Bert, was a very necessary part of the Sunday morning program. Since it was a wise policy of the institution to have all the pupils shave themselves, Bert was given one lesson in shaving. Thereafter, he appeared every Sunday morning with a fresh shave. Apparently he had but little difficulty in mastering the manipulation of the razor, for he never bore any evidence of its mismanagement on his face.

Although Bert realized that it was necessary in general for people to work for a living, he had not the slightest anxiety regarding his own future. He had absolutely no ambitions or desires and took the future as much for granted as he did the past. The suggestion of any occupation or trade by which he might earn a livelihood he responded to by a negative, abashed decline, as though one had seriously proposed a trip to the moon with him. It was quite evident that in the entire twenty-four years of his life, the possibility of ever having to contribute to his own support had never entered his mind. He perceived no relations between himself and his means of subsistence. He felt as if he would always live at his parental home, and, when confronted with the possibility of its discontinuance with the death of his father, he calmly stated that he would then live with his brother in another city.

In the Irwin-Hays Revision of the Stanford-Binet Tests for General Intelligence, he made a mental age of eleven years and two months. He passed all the tests of standard difficulty for ten-year-old blind children. He passed one test (showing the examiner's right hand) of eleven-year-old difficulty, two tests (rote memory and vocabulary) at the twelve-year level, and three tests (rote memory, analogies, and arithmetical reasoning) at the fourteen-year level. Among all the tests given he passed none that required any degree of motor co-ordination. These results indicate that his ability is greater in work that involves rote memory and verbal processes than in any other sort. His fear of attempting what he thought to be too hard a task kept him from trying in some cases where he probably could have responded correctly. For example, in the vocabulary test, he was asked to tell what a lecture is. He stoutly maintained that he did not know, until continued urging and encouragement resulted in his defining it as ''a speech on some particular subject.'' He showed willingness and interest throughout the test.

It is necessary to conclude from the description given of Bert that he is deficient in every respect. He is deficient even in the things in which he is most proficient. It does not help to understand his condition to classify him as being feebleminded. That term is too descriptive to be at all explanatory. In order to make the term feebleminded explanatory, it is necessary to know the conjunction of circumstances which have determined the behavior of the individual. Such conditions can occur within the prenatal environment of the organism, in which case there is produced a congenital case of feeblemindedness. Likewise, conditions can occur in the environment after birth which will produce an individual incapable of responding intelligently in more complex situations. It is not at all necessary to read the description of Bert in order to be familiar with a case of *functional feeblemindedness.* Such cases are encountered every day, ranging in variety from the tramp knocking on the back door for the food he is unable to procure from a too complex social system, to the pet canary singing lustily in her cage hanging in the hall. However, here is the difference between the tramp and the pet canary. The tramp's deficiencies are made apparent in the situation in which he is found, but the canary is a perfectly normal, caged canary that responds intelligently to its caged condition. The deficiencies of the canary would become apparent as soon as it were released from its cage and compelled to shift for itself in the much larger and more complex situation in which the wild birds represent the normal.

From all that could be learned of Bert's infancy, he was a normal baby with the exception of the congenital cataracts. He learned to talk at the normal age and walked at the age of thirty months, which is not unusual for congenital blind children. Throughout his childhood he had no serious illnesses.

In order to appreciate the environment with its unusual conditions, it must be remembered that his family was an extremely religious one. They probably accepted the blindness of their child as a form of punishment sent upon them from God, and it is possible that they regarded him therefore as an object of shame. Their ambivalent attitude toward the child caused them to keep the object of their shame away from society, and their compensated sense of duty to the child caused them to be indulgent with him in the extreme. Two such motives would combine only too well in producing an individual who could not react at all intelligently in any other situation. Since the conditions of the social environment determine the development of all phases of personality, it was inevitable that Bert be precisely as he is.

In spite of the fact that his father was a successful artisan, and his mother a well educated New England woman, and that their eldest son was faultlessly reared and made a splendid appearance both in manner and posture, they failed to correct Bert's frightful speech defects, and to teach him the use of his hands. The pointed dental arch was no insuperable obstacle to his enunciation, for when he had reached the age of twenty-four, it was found that he could learn to repeat passages perfectly, but the correct enunciation which he learned for a specific selection was never employed in his everyday speech. Likewise, it is obvious that he possessed the ability to manipulate his hands in very finely co-ordinated operations. Otherwise he could not have acquired the co-ordinations for shaving and writing with the pencil. It is needless to say that if these tasks had been encountered in the class room or in any other situation, they would never have been learned.

To sum up the conditions which operated in producing Bert's present state, it is necessary to make a clear picture of the small, meagre, vegetative environment his home provided for him the first twenty-four years of his life. Bert was not at all deficient in any respect in his own home. He possessed enough insight and "drive" to master every situation that confronted him. *In the terms of the conditions of his environment, he was living completely and fully*, and, like the released pet canary, *he became functionally deficient only when encountering a larger and more complex situation.*

His home failed to produce two very necessary conditions. It failed to provide objectives, the attainment of which would, by necessity, expand his environment. It also failed to permit the stimulation and incentives to occur which would have made it possible for him to perceive objectives remote from his immediate situation. Bert, as has been shown, is capable of perceiving intricate relationships in the attaining of objectives, such as the act of shaving, piano playing, hand printing and typewriting, when a "drive" is present within him. But his greatest difficulty lies in the fact that he has always been made to consist too exclusively of himself. As he is, his environment cannot impose goals upon him, for to him there exists no relationship between himself and a more complex and larger world.

GLOSSARY

Ambivalent, condition in which two opposed and exaggerated emotional traits or attitudes are balanced.

Automatisms, "nervous" habits; persistent movements of a nervous origin.

Autonomic Perceptions, experiences pertaining to internal organs of the body and to the sexual organs.

Congenital Cataracts, opaque growths, generally of the lens of the eye, commencing before birth.

Dental Arch, arch of the upper jaw.

Erogenous Zones, areas of the body giving rise to sexual excitement when stimulated.

Flatulate, to expel gas.

Gastronomic, pertaining to digestion.

MONTESSORI PEG BOARD	training device used in assisting pupils to learn the shapes and sizes of simple objects; pegs of different lengths or diameters are fitted into corresponding holes in a board.
PHANTASIES	vivid and recurrent day-dreams.
PLAITING	three-ply braiding.
TACTUAL CURIOSITY	curiosity to explore through touch.
TRACHEA	windpipe.
TWO-MANUAL CHORD	chord played with both hands.
VOCAL-MOTOR	verbal; pertaining to inner speech.

READING TWELVE[1]

A STUDY OF FEAR BY MEANS OF THE PSYCHO-GALVANIC TECHNIQUE[2]

Editor's Note

This study of fear by Dr. Nancy Bayley illustrates "mechanical" or "objective" methods of research in the field of emotion, known as methods of *expression*. In contrast with these methods there are methods of impression which require introspective descriptions of emotional processes by the subject himself, or at least some kind of verbal statement pertaining to emotional experiences. Dr. Bayley employed both methods.

The psychogalvanic reflex, so-called, is a change in the physiological processes of the body during emotive behavior. It derives its name, first, from the fact that it occurs with *conscious* behavior and second, from the fact that it can be detected through the use of an electrical instrument, the galvanometer. There are various kinds of galvanometers but in each the general principle is the same. Small differences in the amount of an electric current passing through a magnet induce displacements or vibrations of a string. A beam of light is directed to a small mirror attached to the string and in this way the motions of the string can be photographed. Generally a weak current is passed through some part of the subject's body between two poles or electrodes, and by means of various kinds of resistance boxes (of which the Wheatstone bridge is a type) this current is balanced against electrical potentials in the body. Then, if something happens in the body to offset this balance either by a raising or lowering of bodily resistance to the passage of the outside current, the magnet in the galvanometer is affected, the string is displaced, and a record of the change is obtained. It generally happens that, during emotion, bodily resistance to the outside current decreases, but it yet remains to be established just where this decreased resistance takes place. The most popular theory locates it in the sweat-glands.

The psychogalvanic procedure is of interest and value in a study of the many physical concomitants of emotion, so-called. Every one knows that when frightened, angered, or excited, his heart beats faster, his pulse is stronger, and his breathing is more rapid; and further, that many other organic changes take place, described as cold sweat, drying of the mouth, and sinking in the stomach. He knows, also, how tense his muscles become and that the emotion is revealed in part by facial expressions. The exact relationship of these widespread bodily changes to emotion has been a much discussed problem for centuries. One of the most popular of historic theories, the James-Lange theory, postulates these changes as the cause of the emotion. Unquestionably, taken together, they are one of a number of essential conditions, or, as the author prefers to call them, *features* of emotion.

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[1] Parallels *The Science of Psychology*, Chapter VIII.

[2] By Nancy Bayley, University of Iowa. From the *Psychological Monographs*, Vol. 38, 1928, 1-15, 17, 21-33. By permission. This study was completed under the direction of Dr. Christian A. Ruckmick.

Dr. Bayley's study indicates that there are two types of fear, startle and apprehension, each involving (or attended by) a characteristic change in bodily resistance to weak electric currents. It seems almost certain that her results, and others obtained under Professor Ruckmick's supervision, harmonize with observations subsequently made in the author's laboratory by Edwin B. Newman. When emotionally aroused, Newman's subjects found that there were two characteristically different bodily reactions, the one following the other by an interval sometimes of five or six seconds, but frequently of about two seconds. The first was a definite motor "startle," a sudden onset of tension together with an interpretation of the stimulus-object; the second was a widespread and sometimes very intense organic change occurring with disgust, horror and embarrassment. It frequently happens that the psychogalvanic change does not occur for two seconds or more after the stimulus-object has been perceived. It is the author's belief that a pure "startle" does not involve internal organic changes, but that "apprehension" does involve them. He would class "apprehension" with disgust, horror, and embarrassment, among the emotional reactions that involve a delayed organic change.

The bearing that these organic changes have upon an organismic conception of emotive behavior is discussed elsewhere.[3]

INTRODUCTION

The emotion of fear, like all emotions, is elusive under experimentation. For this reason, and also because it is only comparatively recently that the emotions have been studied experimentally, very little is known of the real nature of fear. Fear is described variously according to the different theories of the emotions which have been advanced. *Hall*, for example, states: "Fear is the anticipation of pain." He goes on to say that without conservation of past experience (both racial and individual) there could be no fear. It causes physiological changes which better fit the individual for fight or flight. In connection with fear he discusses shock: "Here there is no anticipation of pain—but the pain is sprung without warning." "The utterly new and sudden is always dangerous because we lack the apparatus to react fitly to it and so lapse to elementary responses." This instinctive basis is stressed more by *Mc-*

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[3] *The Science of Psychology*, Chapters VI and VIII.

Dougall. Linking the emotion of fear with the instinct of flight, he says: "Fear, whether its impulse be to flight or to concealment, is characterized by the fact that its excitement, more than that of any other instinct, tends to bring to an end at once all other mental activity, riveting the attention upon its object to the exclusion of all others."

Sully defines fear as "the particular emotive reaction which takes place through a sufficiently vivid and persistent representation of possible pain or evil." In agreement with this definition, *Pillsbury* stresses the similarity between emotions, and shows how readily they may be interchanged. "Anger is distinct from fear only in the consciousness of power or weakness toward the intruding man or object, and this changes from moment to moment as the situation is faced."

Somewhat in agreement with Hall's distinction of shock from anticipatory fear, *Ribot* includes two stages of fear—a "primary instinctive unreasoning fear preceding all individual experience, and a secondary conscious reasoned fear posterior to experience." *Dunlap,* in distinguishing between the quickly aroused emotive responses of the striped muscles and the slowly aroused emotive responses of the viscera, shows how they may both be aroused in fear. "In some cases," he says, "the quick component occurs, with the slow component so minimal that we call the complex not fear, but 'being startled.'"

Among the behaviorists, we find *Watson* limiting his description to unlearned fear responses of children. He gives as the causes of fear, loss of support, and loud sounds. The response is "check-of-breathing, 'jump,' or start of whole body, crying, often defecation and urination (and many others not worked out experimentally. Probably the largest group of part reactions are visceral.)." He says of emotion in general: "The shock of an emotional stimulus throws the entire organism for the moment at least into a chaotic state."

From the foregoing statements it is seen that psychologists include descriptions of both mental processes and physical responses. Some stress one and some the other. It is generally conceded, however, that physical response of some kind occurs with all emotion. In this we see evidence of the profound influence of the James-Lange theory of the emotions.

These various definitions raise three important questions: (1) Is fear simple or complex? (2) Are there different forms of fear? (3) To what extent are these forms due to modification?

What fears are inherited as such? We will concern ourselves primarily with an answer to the second question.

The definitions seem to indicate a general inclusive term "fear." In the narrower sense it designates the anticipation of an impending evil whose advent is certain. Dread, however, seems to be descriptive of the contemplation of the possible occurrence of an evil which would be fearful if certain. Terror, then, is extreme fear. Horror adds to fear the implication of aversion, as when watching extreme suffering or torture of another. Startle, fright, and alarm are different degrees of a fear caused by a sudden unexpected stimulus. These are shocks causing a reflex recoil, and are ordinarily short-lived. Watson's description of fear in infants would be entirely of this "startle."

The purpose of the present study is to investigate some of the different types of fear, to determine if possible some basis for these differentiations, and to throw some light on the nature of the emotion or emotions of fear.

EXPERIMENTAL STUDIES

Only a comparatively small number of experimental studies of fear as such have appeared. Many of the older theoretical studies of fear are based largely on the author's own introspections, but these were not made under controlled conditions, and are formed on only one individual's report. An introspective study made by *Conklin* and *Dimmick* gives results of some value in regard to the mental processes in fear. A number of fearful stimuli were presented to three girl students and reports secured from them. They conclude that the "presence of fear does not depend on any particular group of processes such as organic sensations, but rather on some perceptual meaning of the object for the observer." It was found that fear could be produced by giving an imaginal setting before the stimulus was presented.[4]

Physiologists have studied internal bodily changes. *Cannon* devotes many of his experiments to the fear reactions of animals, mostly of cats and dogs. He found identical changes for pain, fear, and rage. These were secretion of adrenin, increased sugar content in blood and urine, more rapid coagulation of the blood, and redistribution of blood to the skeletal muscles. *Crile* shows that the great excitement of intense fear causes evident exhaustion and fatigue of the nerve cells of the cortex.

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[4] We have, incidentally, secured galvanic deflections by merely requesting *O* to feel anger, fear, or some other emotion through verbal suggestion of the name of the emotion. One highly trained musician responded very definitely to this instruction.

In the expressive method of measuring affection there are several procedures. One used frequently in the detection of fear is that of measuring changes in blood pressure. This has been used successfully in the detection of falsehood by *Landis* and *Gullette, Larson,* and *Marston.* The emotion aroused here is probably a fear of being detected, and is anticipatory in nature. Changes in breathing are subject to causes other than emotional to such an extent that pneumographic records are not very accurate. However, work on the inspiration expiration ratio by *Burtt, Landis,* and *Larson* gave some evidence of its possible value in detecting crime. *Moore* has measured emotion in terms of *O*'s ability to do mental computation while experiencing an emotion. The time taken in solving the problems was recorded, and used as an index of the amount of emotional disturbance. He found that fear caused by far the most powerful disturbances in the thought processes involved. This may have been due in part to the fact that his fear stimuli were more effective than the others, and it may indicate that fear is more distracting to mental processes than are other emotions.

Psychogalvanic Studies of Pain and Fear

An objective measurement which has promised to be fruitful is that of the psychogalvanic reflex. The psychogalvanic reflex is a change of electrical conduction through the body which can be measured by means of a galvanometer. By most investigators it is assumed to be due to changes in resistance at the skin. It occurs under emotional conditions and when an emotion is reported by *O.*

In the various studies of the psychogalvanic reflex pain and fear producing stimuli have been frequently used for eliciting emotions. These involve sudden loud noises, pin pricks, slap on face, threat of burn, and the like. The deflections resulting from these stimuli were on the whole of greater amplitude than those occasioned by pleasant stimuli. A few psychogalvanic studies have been made which bear directly on the emotion of fear.

Waller took a continuous reading during a German air raid in London. The subject was a middle aged woman. She sat in an arm chair and read a quiet book. The warning signal caused a slight response, and a very slight response was made to the sound of the defense guns, but a large response was made to the hum of the supposed German planes and explosions. There was also a response to the siren at 1.30 P.M. signalling the end of the raid. This was due to its unexpectedness and novelty.

Blatz studied fear caused by sudden release of support. His *O*s were fastened in a chair which could be made to fall backward suddenly. After three adaptation periods the chair was released without warning. It was released again during subsequent readings. The *O*s showed augmentation of heart beat lasting over 6 minutes, retardation of breathing, and deeper breathing, and a striking increase of e.m.f. with a latent period of from .5 to 3 seconds and lasting from 1 to 6 minutes. Repetition reduced all effects in degree and duration. The *O*s reported fear on the first fall but not on the expected ones. Blatz concludes that a complex organic response and a gross muscular adaptive force is necessary in order that *O* label the experience "fear."

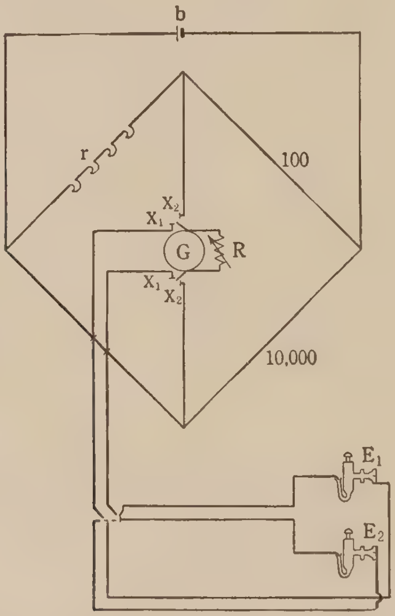

FIGURE 3 (Original FIG. 1)
Schematic drawing of circuit through bridge

The first of these two experiments is for the most part of an anticipatory fear, and the second is a startle. The question remains, are they the same emotion?

In order to study the nature of different types of fear, we have combined the measurement of the psychogalvanic reflex and the respiratory response with introspective reports of the *O*s.

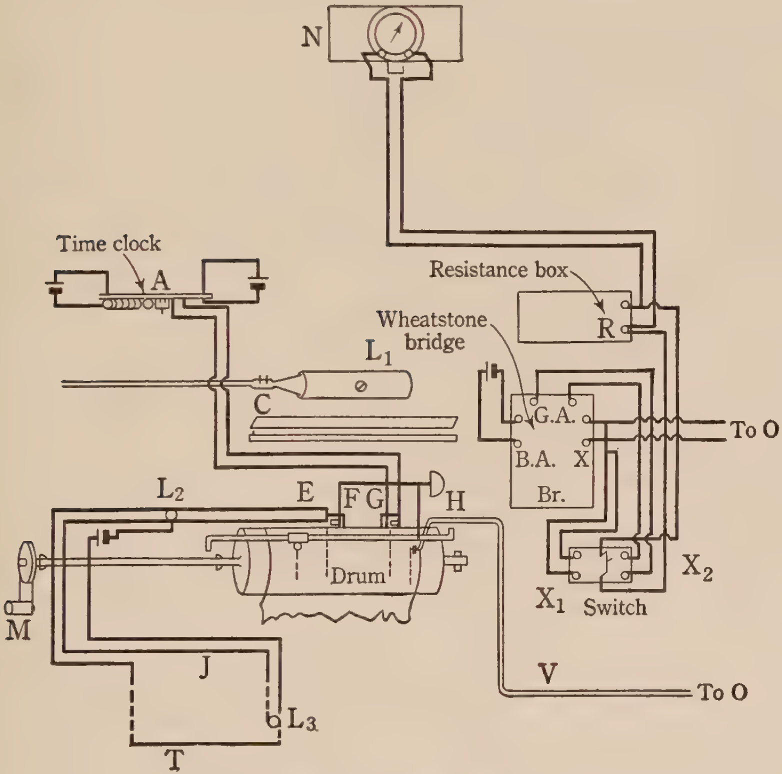
N
Time clock
A
Resistance box
R
Wheatstone bridge
L_1
C
G.A.
B.A.
X
Br.
To O
L_2
E
F
G
H
Drum
M
J
L_3
T
V
To O
X_1
Switch
X_2

FIGURE 4 (Original FIG. 2)
Diagram of apparatus showing wiring

Apparatus. For this experiment we used a Leeds and Northrop (Cat. No. 2285-S) type H mirror galvanometer with a resistance of 12.5 ohms, and damped with a 30 ohm resistance across the two poles. The connections are shown diagrammatically in Fig. 3, while the arrangement of materials and actual wiring are shown in Fig. 4. A variable resistance box (R) was connected across the galvanometer for the purpose of regulating its sensitivity. The current of a single dry cell (b) was applied to the galvanometer through a Wheatstone bridge (Br.). The third arm of the bridge was a variable rheostat (r), while *O* formed the unknown resistance of the fourth arm (O). This circuit is completed by closing the switches at X_2. A circuit through the galvanometer and *O* alone can be made by closing the switches at X_1.

The galvanometer was set up in a closet off from a small room in which the *O* sat. The galvanometer was hung on the wall a meter above the recording material which was fastened to a table. A light (L_1), directed on the galvanometer mirror, caused the excursions of the mirror to be reflected on a transparent scale (C) by means of a lens and an angle mirror. The excursions of the mirror were recorded on a kymograph-drum driven by an electric motor (M) which was geared down to a very slow rate of rotation. The deflections were followed by a fountain-pen in a holder which was made to slide on a rod parallel to the scale, and from which a wire extended toward the scale as a guide (from the holder below E to the scale C above). The pen recorded on white paper which wound onto the drum continuously from a roll. An electric clock (A) actuated a signal magnet (G) every two seconds, causing a jog in the ink-line made by an attached pen. A third pen traced a line which served as a position of reference and also as a signal marker. This pen was similarly attached to a signal magnet (F), and *E* operated it by a button (T) in the outer room at the time stimuli were presented.

During most of the experiments a breathing record was made at the same time as the galvanic record, by a writing point on a tambour at (H) which was connected to a Verdin thoracic pneumograph (V) attached to *O*. There were, then, four lines being recorded simultaneously, a signal line, a time line, the breathing curve, and the galvanic deflection (Fig. 5).

Electric signal lights between the two rooms made it possible for *E* and *R* (Reader) to communicate without disturbing *O*. L_2 was in the same circuit as the signal magnet (F) and was actuated in the outer room by the switch or button (T). L_3 was the light in the outer room which *R* might turn on by pressing the button at J.

O completed the circuit through the galvanometer by inserting his hand between two "calomel" electrodes. These were used be-

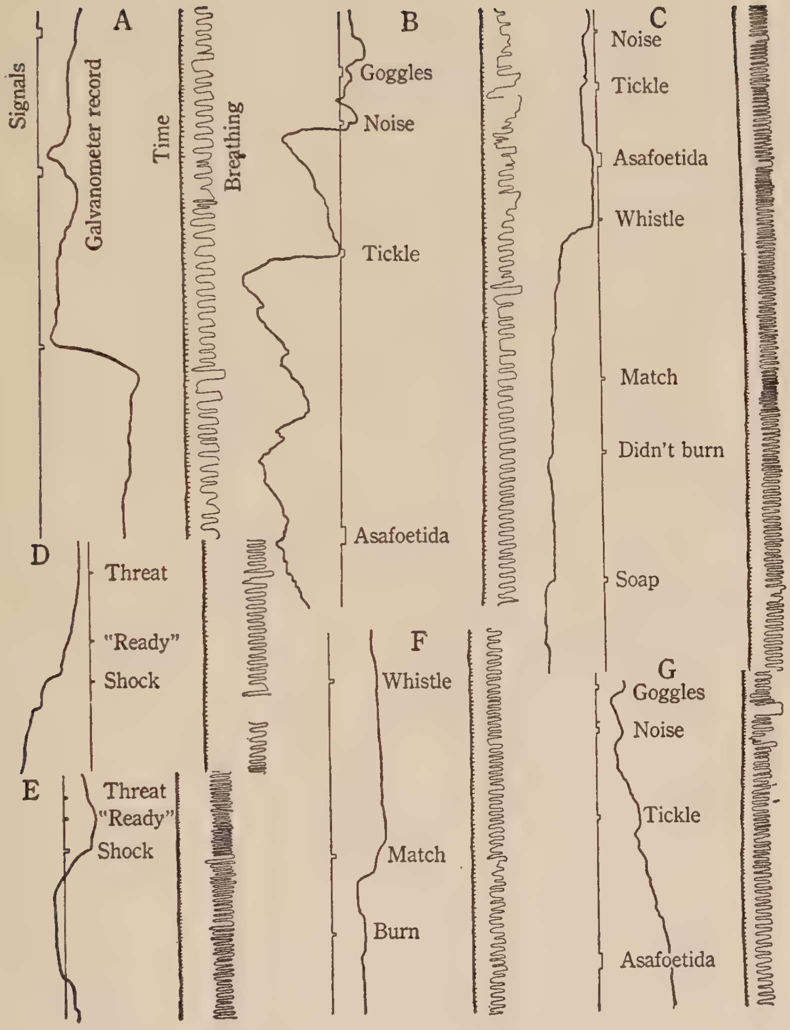

Figure 5 (Original Plate III)
Representative curves

cause liquid insures a more constant contact, and the mercury and mercurous-chloride combination excludes polarization changes inherent in the electrodes. An electrode is shown in Figure 6.[5] The glass container was 14 cm. high with a rubber one-hole stopper through which was inserted a separatory funnel (K) for use in adding liquid. The contact tube (T) projecting at the side was 8.5 cm. long with an opening at (F) 6 mm. in diameter. To the bottom of the container a connecting tube (E) was attached from which a lead (H) went to the galvanometer. The connection through the glass was made by means of a platinum wire (D). In the container a layer of mercury (A) was covered

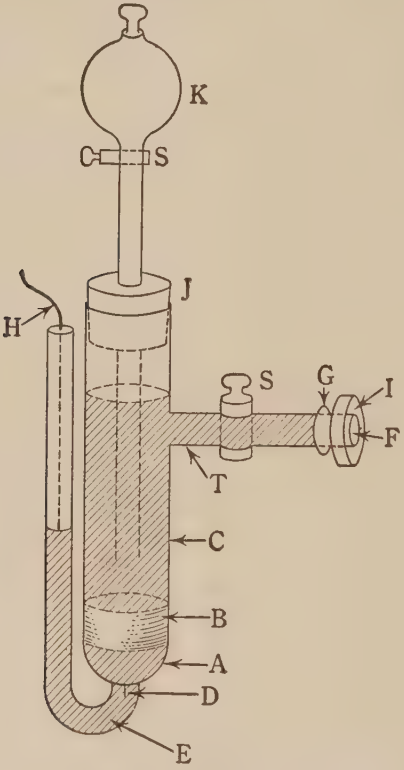

Figure 6 (Original Fig. 3)
Electrode

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[5] Professor J. N. Pearce, of the department of physical chemistry, very kindly designed the electrodes.

by a layer of a paste of mercury and mercurous-chloride (B). The rest was filled by a 1 per cent. sodium chloride solution (C). This latter formed a liquid contact at (F). The flow of the liquid was regulated by means of stopcocks at (S) and (S). The two electrodes were fastened to the palm and back of *O*'s hand by means of wooden holders placed back of the shoulder (G), and held together by strong rubber bands. The electrodes fitted into a wooden box which rested on the arm of a Morris chair in which *O* sat. The pressure of the contacts was relieved by brass discs 2½ cm. in diameter and ½ cm. thick with a hole in the center just large enough to slip over the glass tube at (F). This formed a larger pressure surface on the hand. It was also possible to connect these discs directly to the galvanometer if desired in place of the liquid electrodes to give an electrode of the type used by many investigators.

OBSERVERS

Of the twenty-five in this experiment eleven were women and fifteen men. The women are designated as *By, Ca, Ge, Ha, Han, Ho, Pat, Pa, Ru, Sp,* and *Wa.* The men were *Ba, Bl, Be, He, How, Lu, Me, Se, Th, Tr, To, Wag, Wi, Wo,* and *Yo.* Most of them were students in the advanced laboratory class in psychology, and had therefore had several courses in psychology. They were graduate students with the exception of *Ha,* a professor in physical education, and *Pat* and *To,* who were juniors. Most of the *O*s had had some training in introspective observation, while *By* and *Pat* had considerably more training than the rest. They ranged in age between approximately eighteen and forty-five years.

PRELIMINARY ADJUSTMENT AND INSTRUCTIONS

O was seated with the left hand fastened into the electrodes on the chair arm, his hand having been first cleaned with a piece of cotton dipped in alcohol.[6] For the later experiments a Verdin thoracic pneumograph was first adjusted on *O* for the breathing record. The stopcocks on the electrodes were opened, completing the circuit. *E* then gave *O* a typewritten paragraph of directions and a list of names of emotions with accompanying definitions to read. The directions read:

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[6] This was done to remove all impurities from the hand and to remove any excess moisture that might allow conduction over the surface of the skin.

> Make yourself comfortable and sit quietly. Attend to whatever happens as you will be asked afterward to give a report of how each situation affected you. Take note of the kind of feeling produced, its intensity and duration. Name it if possible. Rate the intensity on a scale of 10: with 10 very intense, 5 moderately intense, and 1 indifferent. Note also associated ideas and imagery and any physical or physiological changes, as muscle strains, and relaxations and their location, as dryness of throat, moistness of palms, organic sensations, etc.

The definitions of emotions were given because it was found that the untrained *O*s were at a loss for words to describe their affections. The list was not nearly comprehensive, but was fairly full in the emotions which were related to fear, and which were of greatest importance in this study. Most of the definitions were taken from the Funk and Wagnall dictionary. They were verified and in some places slightly changed on the basis of definitions of a number of psychologists.

As soon as the galvanometer circuit was closed the shunt was adjusted and the Wheatstone bridge was balanced. With the writing points inked, and the time-clock going, *R* started the kymograph motor and took the record. *E* blindfolded *O* and gave the stimuli, actuating the signal magnet with each stimulus.

The records of the deflections were made on the drum by a reader who moved the pointer back and forth on the rod to coincide with the mirror deflections. Four graduate students in psychology were trained and made all the readings with the exception of a few made by *E*.

AFFECTIVE SITUATIONS

A number of situations were arranged with the stimuli in a definite order. This order was followed for most of the *O*s, but not rigidly. Some *O*s observed five or six times, while others observed only once, so that quantitatively there are more records for some stimuli than for others. In addition to this, the effectiveness of the stimuli varied with the individual *O*s. This made it advisable to vary the situations at times.

The first situation was designed to call out rather elementary reactions, the stimuli being very simple. They were in order, noise (dropping of iron weight), tickle on lips with soft brush, asafœtida under nose, shrill whistle, and holding a lighted match till it burned the finger. As a final stimulus *O* was told that nothing more would happen, but to sit quietly and rest for a minute. This is designated in the tables as "all over." The later situations involved more complex ideas and associations. Gruesome stories were read to work up a mental attitude in which emotions would be easily aroused.

The second sitting began with a "gong" (an A^1 tuning fork on a resonator). *E* then read aloud a gruesome story, "Solange," by A. Dumas. After this cool air was blown across *O*'s face with a fan; and finally a .38 revolver loaded with an extra loud blank cartridge was fired about 4 feet from *O*.

In the third situation *O* read to himself a newspaper clipping which described in detail a severe punishment of criminals (referred to in the tables as "Cat o' nine tails"). Then *E* said to *O:* "I want you to estimate in inches the distance around your right wrist and the distance around your head at the largest part, and then tell me the difference between them. For each inch mistake you make you will receive a strong electric shock. I have a tape measure here and will make the measurements when you have given me your answer." The shocks were applied in accordance with the threat. This was the only sitting in which *O* was not blindfolded.

In the fourth situation *E* read aloud "The Cask of Amontillado," by E. A. Poe. Then *O* was told to multiply mentally the following problems and tell *E* the answer. They were: 4 × 13, 15 × 12, 12 × 18, 13 × 17, 236 × 27, 192 × 14, 364 × 27, 196 × 124. *O* was encouraged on the first ones but on failure with the fifth he was given an electric shock and threatened with another shock for each wrong answer. *E* continued this only until *O* had received punishment for this and had corrected one problem.

Situation five started with an oral quiz. *E* said, "Professor Ruckmick has asked me to rate you in laboratory work, so I am going to take this opportunity to ask you some questions about laboratory apparatus and find what you have learned this year." *E* then asked a number of questions which were hard enough that many of them could not be answered by the *O*s. After the quiz a bottle of formaldehyde was held under *O*'s nose, and then his hand put into a jar of soft soap. *E* then told *O* she wanted a blood test, cleaned the end of *O*'s finger with alcohol, and pricked it. This is referred to as "blood" and "prick." *O* was then told that the quiz was a farce and would not be used in the grades. The final stimulus was a piece of chocolate candy.

A few other stimuli were used on one or two *O*s. Two Victrola records, Kreisler's *Old Refrain*, and the *Erl König* sung by Schumann-Heink, and an article describing cattle drowning at sea, were presented as stimuli to several *O*s. Three *O*s, *Ba*, *Ho*, and *Pat*, were given the gun to shoot themselves when *E* signalled.

Ten of the *O*s who observed only once were stimulated with noise, tickle, pin, asafœtida, whistle, match, hand in soft soap, and gun. This series of readings was made after the others. In all the experiments except in situation three, *O* was blindfolded throughout the experiment. At the end of each series *O* gave his introspective report verbally to *E*, who wrote it down as it was given.

Deflections occurred a number of times at the beginning of the experiment before the series of situations was started. The uncertainty of what would happen next seemed to be an emotive situation here. These deflections are indicated in the tables under the stimulus column by the word "beginning."

Treatment of the psychogalvanic results. Figure 5 shows samples of the records as they were made during the observations. They lend themselves to a certain number of comparisons and assumptions as they stand, but cannot be compared in regard to absolute magnitude of the deflections, for at each observation the bridge was balanced at a different resistance, as *O*s vary in body resistance.

In order to make comparisons of the reactions, the magnitude of each deflection of an *O* was multiplied by the resistance as measured by the Wheatstone bridge at the experimental hour. This made all the readings for one *O* comparable if the sensitivity of the galvanometer was the same. The arithmetic mean of these deflections was then computed and the deflections translated into per cent. of the mean deflection,[7] giving a value which could be used in comparing the relative degree of reactions of all *O*s and with varying sensitivity in the galvanometer, to the various situations. For different galvanometer shunts the percentages were computed separately as for different *O*s. Table I gives the per cent. values in descending order of magnitude for each *O* with the accompanying stimulus. Since a large number of deflections was not secured from each *O* no exact statistical treatment would be valid, but the results give sufficiently large differences to indicate tendencies.

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[7] Smith in Appendix II explains and justifies the use of this method for comparisons of the psychogalvanic reflex for different *O*s.

From Table I [one sample given here] the scores in terms of per cent. of the mean for each stimulus were collected and the arithmetic mean of these scores computed. These are listed in descending order of magnitude in Table II. An examination of

TABLE I

GALVANIC RESPONSES OF *O* LISTED IN DESCENDING ORDER OF MAGNITUDE

Score: Per Cent. of Mean	*Stimulus*	*Response*
451	after shot by *O*	*Ho*
314	gun	very intense startle
248	shot by *O*	very unpleasant
243	after candy	expected?
243	whistle	awful, fear
207	soap	indifferent, then dread that it was dirty
189	gun expectation, shot by *O*	anxious (dread of loud noises)
171	formaldehyde	tiredness, strong associations
139	shock (problem)	didn't bother much
131, 63, 95	"Erl König"	rage, tense
125	whistle (2nd)	fear
121	noise	surprise and alarm
105	tickle	neutral
99	soap from fingers	
87	end "Cat o' nine tails"	
74, 42	"cattle"	anger, shrinking
71, 31	problem	
50	arithmetic	baffled feeling
50	arithmetic shock	didn't mind much
40	"all over"	
39, 27	"Cat o' nine tails"	slight shudder, disgust
37	quiz	chagrin, astonishment
37	"Amontillado"	enjoyed, tense
34	gong	unpleasant then pleasant
34	fan	
33	asafœtida	almost nauseating
27	burn	slight pain
22	blood	didn't bother
11	match	no fear
7	whistle (3rd)	absolute fear

Table II shows that the "startle" stimuli, the gun, the whistle, and the noise fall well at the top of the list. So also do the electric shocks which cause a "startle" reaction. The pin prick stimuli, which cause physical pain, are also high. The burn, however, is considerably lower than is the expectancy of it (match). The items at the bottom of the table seem to be either things which are very slightly affective or which are conditioned on experience and may or may not be affective, depending on the *O*. From this we may assume that the fear stimuli which more generally elicit the psychogalvanic reflex are sudden intense stimuli which are described affectively as "startles." The small number of cases for each stimulus must not be overlooked, but the differences are great enough in the whole table to allow for the above interpretations.

TABLE II

DESCENDING ORDER OF DEFLECTIONS ACCORDING TO STIMULI FOR ALL *O*s

Score: Per Cent. of Mean	*Stimuli*	*No. Cases*
349	gun shot by *O*	2
256	expectation of shot (by *O*)	2
240	gun	12
208	shocks (problem)	8
167	whistle	16
122	prick	3
119	noise	15
112	arithmetic shock threats	12
110	pin	5
102	formaldehyde	7
99	shocks (arithmetic)	4
92	quiz	6
87	match	15
79	tickle	14
69	"Erl König"	5
64	expectations	23
63	"cattle"	4
53	burn	14
51	"Amontillado"	3
50	gong	7
50	goggles	17
47	"Cat o' nine tails"	7
43	soap	13
40	problem	10
36	asafœtida	16
35	blood	4
33	exam. farce	6
31	"Solange"	7
24	candy	7
18	"all over"	25
9	fan	4

A more significant treatment of the data is shown in Table III. Twelve emotions were listed and the scores for each emotion as reported by every *O* were combined and the arithmetic mean was taken for each emotion and for deflections which were reported as non-affective or neutral. The mean scores are again tabulated in order of descending magnitude. By far the greatest deflections are occasioned by stimuli causing emotions which are described as startle, surprise, or shock, the mean score being 169. The next greatest reactions with a mean score of 123 were described as fear. This, according to reports, seems to be very much the same as apprehension or dread, which, with a mean score of 108, is classed fifth. If they are considered together they rank third. These differences, however, are slight and are of little significance. The important fact is that the "startle" emotions appear much greater than do the apprehensive fears in so far as they are measured by the psychogalvanic reflex. There is no absolute standard by which it can be determined whether the stimuli used for production of apprehensive fears were sufficiently intense to be comparable to those causing startle. An examination of the introspections may give some cue to this.

TABLE III

DESCENDING ORDER OF MAGNITUDE OF DEFLECTIONS ACCORDING TO REPORTED EMOTIONS

Per Cent. Mean	*Emotions*	*No. Cases*
169	startle, surprise, or shock	53
123	fear	14
111	fear and apprehension combined	89
109	pain	19
108	apprehension dread, or expectancy	75
104	humiliation, embarrassment	3
80	anger, irritation	9
72	gruesome horror	5
60	disgust or repulsion	14
45	relief	15
35	neutral "didn't bother"	23
26	pleasant	25
14	slightly unpleasant	6

On the whole, the nature of the stimuli were such that startles and fears would tend to be more intense than other emotions, so the lower end of this table cannot be accepted without reservation. Other studies, however, have reported that fear stimuli cause the greatest deflections.

Situations which were reported as not affective show in this table a considerable amount of deflection, averaging 35 per cent.[8]

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[8] Syz has shown that the subject's report of the presence or absence of an emotion in response to association words, is often unreliable.

There are two factors which seem important in causing this. The whole situation was calculated to induce fear, and these reactions may be due to expectation of something so much worse than the actual stimulus that it in itself produced no conscious emotion but came as a relief from the apprehension caused by the uncertainty of what would happen. In some cases *O* may not have been conscious of an emotional reaction because through training he had acquired the attitude of not being affected by certain things. Many of the reports were worded in such terms as ''that wasn't bad'' and ''that didn't bother me much,'' indicating a somewhat defensive attitude toward the situation.

A few other general tendencies are found by casual inspection of the records themselves. The tracings in Figure 5 show that most of the stimuli of a ''startle'' nature cause a very abrupt change of resistance. The whistle in A, the noise and the tickle in B, and the whistles in C are examples of this. On the other hand, ''anticipation'' or ''expectancy'' deflections are as a rule more gradual. This is noticeable in the shock threat in D and the match or expectation of burn in F. An inspection of all of the records shows that of forty-eight deflections which could be characterized as very abrupt changes, thirty-six, or 75 per cent., of them were caused by stimuli which were described as shocks or startles. This is not complete agreement, however, and many of the emotions described as startles are not accompanied by a reflex of this character. On the other hand, abrupt changes which were caused by other than ''startle'' stimuli usually occurred when *O* was excited and tensely expecting something to happen. It indicates that on the whole ''startle'' emotions are much more sudden than apprehension emotions. The latter would appear to accumulate gradually with more complete realization of the situation.

During a sitting, the level of resistance usually shifts continuously in one direction or the other. Comparisons of the records with introspective reports show that in general the direction of shift coincides with the amount of excitement or apprehension felt by *O* throughout the test period. Readings showing continued shifting to the left as in B, C, D, and F, in Figure 5 indicate a lowered resistance. The psychogalvanic reflex is always in the nature of a lowered resistance. The reports of these *O*s correspond with the inference that the affective tone was heightened throughout the experiment. *Se* reported for the experiment from which sample B was taken, a feeling of anticipation which lasted until the end. *Tr* for example C was continuously expecting an electric shock in his left hand where the electrodes were attached. *By* in D felt much more excited and wakeful after the shock than before, and in E, though feeling tense during the whole reading, was too fatigued to keep her attention long on the emotional situations, and they were of relatively short duration. In F, *Wi* also reported a good deal of apprehension before each stimulus. This apprehension became greater toward the end. For G, which shows the opposite trend, *He* made an attempt to control his general attitude as he would under hospital conditions, and found that he became more normal toward the end. *How,* whose curve was similar to that of *He,* reported no intense emotions and appeared to be very little excited. Here again there are exceptions to the tendency. Some *O*s who reported a continued emotional state did not show it in the record. This may be due to individual differences in skin resistance and the amount of polarization in the skin, or, as indicated in sample E, to bodily fatigue. During the reading of stories, with a few exceptions, the resistance level gradually increased. Very few of the *O*s felt apprehension during these stories but reported instead repulsion, horror, and feelings of gruesomeness which were rather depressing, or else merely a calm relaxed condition.

A number of rest curves were made with the *O*s assured that no stimuli would be given. These were taken over 5 and 10 minute periods and they invariably resulted in a general shift to the right. It seems reasonable to assume, then, that continued emotional tone in fear situations is evidenced by a general lowering of resistance.

The reactions of *O Ca* were exactly opposite from those of all the other *O*s (A, Figure 5). If reversed, the deflections are typical deflections to the stimuli given. As *Ca* could not be secured for a second experiment, no check could be made on the reactions. It is offered here as an exception which without further evidence is of little significance.

Variations in resistance on different days and at different times of the day for the same *O* have been noted by most of the investigators of the psychogalvanic reflex. Such experiments as those of Waller and of Farmer and Chambers show periodic changes during a twenty-four-hour day. These were thought to be due in part to variations in fatigued and rested conditions of the *O*s. We made a single observation with *By* comparing reactions while in a rested and a fatigued condition. Sections of the results of this trial are shown in samples D and E of Figure 5. They were obtained during one evening between 7.30 and 10.00 o'clock. For the rested experiment (D), *O* and *E* carried on a pleasant conversation while looking at prints of oil paintings, and eating candy. *O* then lay relaxed in a comfortable position for about five minutes after which she went into the experimenting room and experienced a fearful situation while connected in series with the galvanometer. The stimuli were first a threat of a strong electric shock, then *E* said "get ready," and after a few seconds the shock was given. For the fatigue series *O* did vigorous setting-up exercises until physically tired, and then was subjected to a difficult oral quiz and made to feel very discouraged by her inability to answer the questions satisfactorily. Galvanometer readings were then taken for the same stimuli. The results of this are shown in E (Figure 5).

The bridge was balanced at exactly the same resistance for both experiments. The "rested" series shows an expectancy curve which started with the threat of the shock and continued during the whole experiment. The reaction to the shock itself was rather small and is not distinct from the general expectancy trend. *O* reported with the threat of shock anticipation and slight dread with muscular tenseness just above the knees and twitching of the hand where the shock was to be applied. When told to get ready she became more tense and felt a decided fear of the shock, with involuntary twitchings spread over the whole body. The shock was painful and caused an involuntary reflex jump of the whole body with quick intake of breath. Afterward she calmed down gradually but felt much more wakeful than before.

The fatigue series shows no reaction to the threats but a strong reflex to the shock. *O* reports for this series fatigue and general tenseness with inability to relax. With the threat there was a slight anticipatory dread, but the muscles which were already tense, did not seem to change. With the warning to get ready, however, fear was experienced and the muscles tightened. The shock caused a reflex jump and was more painful than before. It caused a sharp pain in the head about the temples which lasted some time. The fatigued condition seemed to *O* to make it hard to attend to the stimuli, and the emotions were not continued but soon left. This last statement seems to account for the differences between the two records. Anticipation was lacking on account of fatigue and failure to attend for long to the emotional situation. For this reason too, the resistance did not remain lowered as in the rested experiment.

RELATION OF THE BREATHING CHANGES TO THE PSYCHOGALVANIC REFLEX

Because in describing startles and alarms the *O*s mentioned breathing changes far more frequently than they did in other emotions we decided to make a pneumographic record with the galvanic record to determine if the breathing changes were more frequent with startle reactions. The breathing curves on the records shown in Figure 5 are typical of all of the records. The changes in breathing show some correspondence with the psychogalvanic reflex, irregularities either in rate or in depth of breathing or both occurring with most affective stimuli. This, however, is not invariable. Breathing disturbances occur where there is no galvanic deflection, and deflections occur with no breathing change. The changes occasioned by anticipatory emotions are not unlike those caused by startles, and they are just as prevalent. Types of breathing changes seem to have no influence on the galvanic curve, nor do they show in themselves patterns which are characteristic of different emotions.

INTROSPECTIONS OF FEAR

Although the *O*s were not introspectively trained, their reports show distinctions which would indicate some fundamental differences in kinds of fear. This is evident from reading the reports when they are grouped into startles, apprehensions, and pains, as below.

FEAR PROPER

Ba. *shock*—caused jump and contraction but no emotion.
  whistle—slightly stunned, inhibition of everything, then motion started in thorax and moved down, similar to dream of falling but not so intense; sort of a fear.
  noise—caught unawares, for two seconds had a real emotion, "got sore," turned toward the sound and caught breath.
  gong—slight startle felt in chest, moved down a little but didn't last long.
  gun—stunned at first and real fear, when realized what it was the whole affective condition gradually disappeared.
  gun shot by O—scared, surprise complete (had thought it not loaded) jumped.

Bl. *noise*—not very intense because wasn't expecting it. There was a sense of muscular tightness around the ears.

gun—shock, general muscular contraction and drawing together, no fear but intense surprise or startle. Gradually relaxed when realized what it was. Noticed heart beat faster, having slowed with the shock.

shock—surprised or startled, muscles of arms and chest contracted, quick inspiration with exclamation of pain, fingers were damp with perspiration.

whistle—very startling, muscular contraction especially in arms and chest, then heart began to beat faster.

shock—jerked away, muscular contraction especially in arms and chest.

APPREHENSION

Pat. *expectation of gun* (*shot by O*)—intense fear; hand got very clammy and rather shaky; pressure in chest; sinking in pit of stomach; tensing of muscles.

Ru. *match*—anticipation partly fear; tendency to drop it; and dropped before really burned much.

problem—the anticipation was definitely fear; mostly of the shock; caused more close attention.

arithmetic—dreadful; afraid to try figuring without pencil; nervous; sure couldn't get right answers so really afraid; held breath a great deal; went through more mental anguish than in any other experiment.

To. *quiz*—fear; chill passed over; felt trembly all over; mind seemed confused.

expectation after quiz—almost in panic; was fussed at failure.

Wo. *problem*—dread of shock when knew couldn't save self; intense horror or dread of the shocks.

PAIN

Ba. *burn*—very little pain, jerk, then vanished.

pin—sharp feeling; not emotion but pain; unpleasant; slight muscular tension in right arm; tendency to draw away.

Bl. *pin*—annoyance; some pain; surprise; involuntary withdrawal of hand.

burn—slight pain; no emotion; withdrawal.

He. *pin*—largely sensational pain; no apprehension; felt secure under conditions of experiment.

Lu. *shocks*—stinging pain and unpleasant.

Me. *prick*—pain.

Pa. *burn*—sharp pain; started and jerked hand away.

The introspections showed that with startles attention seems to be directed more to the intense physical reactions. There were also frequent references to tingling of body and various parts of the body. This is probably evidence that the muscular contractions were intense and sudden. On the other hand reports of dreads and apprehensions had less of the organic in them and more of the ideational. Many *O*s definitely stated that the startles were not fears but were purely physical reactions.

In anticipation the physical reaction when it was present developed more slowly. It depended on a perception of the situation as fearful or harmful.

The pain reactions were very similar to the startles. Perhaps the main difference here is that they are occasioned in a different sense department than are the startles caused by noises. They might better be classed under the startles.

Comparison Between Introspective and Psychogalvanic Results

The psychogalvanic reflex which is characteristic of "startle" fears is a very abrupt rise which is ordinarily of greater magnitude than the reflexes caused by other emotions. In correspondence with this the introspective reports indicate sudden and intense organic and muscular reactions to such fears to a far greater extent than to other emotions. It may be possible that the absence of fear in startles is only apparent, because recognition of the situation as harmless comes so soon under laboratory conditions, but if fear is apprehension of a harmful event, then startle is quite possibly qualitatively different. These introspective distinctions agree with the results of the psychogalvanic reflex in the two types of "fear." Introspectively startle fears are very short-lived, more so than the duration of the reflex would indicate, except for the more lasting physical effect. This is apparently because of the perception of their cause and their harmlessness. Apprehensive fears, because of the longer duration, and the perceptual element of realization of imminent danger seem to be qualitatively different.[9] The only physiological difference which could be inferred from this study is the suddenness of the reaction of the startles as opposed to the gradual reactions of apprehensions.

Conclusions

1. The most intense psychogalvanic reflex deflections occurred in response to stimuli which were described as startles, shocks, and alarms. Whatever the physical reaction which occasions the change in skin resistance, it seems to be greater for the most

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[9] Watson considers only sudden fears of a startle nature. His experiments point to the possibility that all apprehensive fears (including fear of snakes and the like) are learned and depend on perceptions built up through conditioning.

part in response to sudden unexpected stimuli. This is shown to be true both when the reactions are classed according to stimuli, and to introspective reports of the emotion experienced.

2. The gradual change of resistance which occurs during a single sitting is affected by the state of apprehension or ease of the *O*. Continued excitement and apprehension cause the general bodily resistance to decrease independently of the separate deflections. When no expectation is present bodily resistance tends to increase. In connection with this there is some slight evidence based on a single experiment with *By*, that body resistance increases rather than decreases when *O* is fatigued, even under considerable excitement.

3. Reflexes which are caused by startling stimuli are temporarily more abrupt than those caused by anticipatory fears. The response to the startle is complete from the beginning, while the apprehension of danger grows comparatively slowly with perception and realization of the situation. This is not invariably true, but is predominantly so. The exceptions usually occur when the *O* is in a tense and excited condition.

4. Introspectively startles are differentiated from apprehensive fears in a way which corresponds with the temporal character of the reflex. The reports indicate sudden and intense physical reactions for startles with almost immediate realization and subsequent calming and relaxation. Anticipations and dreads are described more frequently in mental terms denoting expectancy and increasing uneasiness. The physical reaction, however, is present in the anticipations and is often described, but does not appear so prominently as in the startles.

5. Breathing changes usually occurred with emotional reactions, but these changes were not differentiated in nature with regard to different kinds of emotions.

6. Startles appear to be textually different from apprehensions because they are of very short duration, and because they are reflex rather than perceptual in nature. In startles the perception occurs after the reflex response, and when the sudden stimulus is not accompanied by danger the perception of the situation relieves the mental anguish, leaving only the sensations of the physical reaction. In apprehensions the danger usually is perceived more slowly and continues over a comparatively long space of time.

7. This study indicates, both quantitatively and introspectively, that there are at least two types of fear, namely startle,

and apprehension. They are distinguished by the difference in the immediateness of the reaction, and in its duration.

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GLOSSARY

ADRENIN, secretion of the adrenal glands, two small glands located just above the kidneys; these glands are ductless; the secretion is assimilated directly into the blood stream.

AFFECTION, relatively simple forms of emotional experiences known technically as feelings; pleasantness and unpleasantness.

ASAFŒTIDA, gum-resin giving off an unpleasant odor.

"CALOMEL" ELECTRODE, cap containing solution of mercurous-chloride.

CORTEX, outer layer of grey matter of the brain, popularly considered responsible for consciousness and intelligence.

E, experimenter.

INSPIRATION-EXPIRATION RATIO, ratio of the time of inhaling to the time of exhaling, the first divided by the second.

KYMOGRAPH DRUM, a cylinder revolved by clockwork; recording paper is fastened around the drum.

O, observer.

ORGANIC SENSATIONS, sensations, often spoken of as feelings, conditioned by sense organs (minute sensitive nerve structures) in the internal organs of the body; illustrated by thirst; hunger; pressure and pains in the abdomen; "sinking" feeling in the stomach, probably a contraction of the diaphram between the chest and abdominal cavities.

PNEUMOGRAPHIC, pertaining to a pneumograph, an instrument used in recording breathing.

POLES (in this case), terminals.

THORACIC, pertaining to the chest region, or part of the body between the neck and abdomen.

VISCERAL, pertaining to the viscera, or vital organs of the body.

READING THIRTEEN[1]

ANIMAL BEHAVIOR AND INTERNAL DRIVES[2]

Editor's Note

The Instinct-emotion Problem.—The modes of animal behavior investigated by Dr. Richter and described in the following article have long been recognized in a general way under the name of "instinct" or "inherited behavior." The term instinct, however, has fallen into more or less disrepute among psychologists for the reason, first, that historic definitions of it that were intended to be scientific turned out to be objectionable, and second, instinct became popularly recognized as an explanatory term when it is not in any sense explanatory.[3] In early Greek and Latin literature it originally meant "urge" or "striving," something akin to the modern psychological use of the word "drive." Through the influence of biological science, the term came to mean specific inherited modes of response, and to account for these modes of response, definite, preformed pathways or patterns of nerve cells were postulated. Nerve cells are positive facts, but predetermined, organized patterns of a type necessary to explain the inheritance of multitudinous forms of behavior are not facts; they are highly doubtful and illogical hypotheses. The modes of behavior called instincts are facts—fighting, fleeing from danger, caring for offspring, nest building, seeking food. But to define them as instincts is again to assume too much, for it is doubtful if such modes of behavior are inherited in any strict sense of the term, any more at least than processes not regarded as instincts are inherited, as for example, reasoning. Instinct, historically, means the antithesis of reasoning and insight. By some it was thought to be a mechanical, blind activity; by others, a purposive activity with no perception of a goal. Now we know that it is a conscious goal-activity and that, as such, it is intelligent behavior.

Again, the term instinct is objectionable for its historic connections with the concept of emotion. Psychology was for a long time dominated by a philosophical theory known as dualism. This theory made a sharp distinction *of kind* between mental and physical processes. It is now believed by many scientists and philosophers that this distinction is artificial and that physical and mental processes are different in *form,* not kind, for they seemingly follow the same dynamic laws. This is known as a *monistic* as opposed to a dualistic theory. Meanwhile, instinct was defined as a physical process and emotion as a mental process, but since the two seemed always to occur together (evidence in itself that they were one and the same activity) numerous conjectures were offered in an effort to explain their relationship. The task was an impossible one for the reason that dualism was an impossible theory. Needless to say, no theories of the instinct-emotion relationship solved the problem.

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[1] Parallels *The Science of Psychology*, Chapters VI, VIII, or IX.

[2] By Curt P. Richter, Psychological Laboratory, Phipps Psychiatric Clinic, Johns Hopkins University. From the *Quarterly Review of Biology*, Vol. 2, No. 3, 1927, 307-343. By permission. All rights reserved to holder of original copyright.

[3] One can, of course, make a term mean anything one wants it to mean, but it is confusing and therefore inexpedient to attempt a radical change in the definition of an old word, just to save the word. It is better to invent a new one. See discussion in *The Science of Psychology*, 159-175, 217-224.

Today the terms "drive" and "urge" have been revived and substituted for the term instinct in an effort to avoid these historic dilemmas. But their revival has brought with them their old vitalistic connotation, that of a specific, propelling force driving an organism on or attracting it. In other words, there is the assumption of a push from behind or a pull from the front.[4] Such views attempt to explain a part in terms of another part; they neglect the dynamic relationship of parts to a whole. Hence the terms "drive" and "urge" do not lend themselves well to organismic usage. Moreover, there is a strong temptation to employ "drive" and "urge" to mean a specific performance such as an urge *to eat* and an urge *to be with one's fellows.* This usage is confusing, first, because it breaks over into the field of conscious goal-activity where the terms insight and intelligence are more appropriate, and second, because it implies that the *to eat* or purposive aspect of the urge is something different from intelligent behavior, when, after all, it is the same thing. To make the "urge" specific, therefore, opens the term to all of the objections that have been raised against instinct. The term "tension" is to be preferred. "Tension" cannot be used in a specific sense and therefore demands an account in language that describes its conscious aspect, like "desire," "wish," "purpose," "intention," and "effort toward a goal." Moreover, *"tension" is intelligible only in an organismic sense, as an aspect of a configuration.*

This discussion is not intended as a criticism of Dr. Richter's use of "drive," but as a supplement, and a means of relating his research to the point of view from which these readings have been selected.

In common with certain behavioristic redefinitions of the behavior called instinct,[5] the organismic view regards such behavior to be conditioned in part by *intra-organic* stimulation or stimulation from within the organism. This form of stimulation induces tensions and therefore has much to do with the determination of goals. It is part of the total situation that is setting up a configurational reaction. Dr. Richter's investigation, and those of others whom he quotes, are concerned with various forms of intra-organic stimulation and have thrown a great deal of light upon a hitherto much misunderstood problem, the causation of so-called "instinctive" behavior. This paper is relevant not only to the problem of "instinct" and emotion but also to the problem of learning, for the learning process often depends, in part, upon motivation through intra-organic stimulation. Studies like those of

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[4] For this reason the author suggests that such conceptions be classified as types of "one-line" functionalism as opposed to organismic functionalism.

[5] Especially of the type found in Perrin and Klein, *Psychology: Its Methods and Principles*, New York, Holt, 1926.

Dr. Richter's show that motivation is a process of increasing the organism's tension toward a goal. It is not an act separate from the learning process itself and can only in a popular sense of the term be regarded as a cause of learning. The same conditions that account in part for emotive behavior account in part for the learning process.

One of the most fundamental of all the phenomena which characterize animal life and distinguish it from plant life is the spontaneous motility of the animal organism. A few plants, to be sure, especially certain forms of marine vegetation, do move about, but these few are exceptions in the plant kingdom. The activity of animals, on the other hand, although it varies widely in form and extent from species to species, is an ordinary phenomenon which one always anticipates under normal circumstances. We may ask, then, what it is that sets off the diverse performances which animals display. Ordinarily we think of most of their activity as being due to some form of external stimulation. We know, however, that all animals, from the lowest uni-cellular organism to man, are active when all external stimuli have been eliminated. And since this spontaneous motility, just as any other kind of motility, must have a definite cause, it must be due to some natural factor within the organism. Many workers have chosen to call it "voluntary" activity, presumably because of the common belief that the "will" to do is the origin of the action. We believe, however, that spontaneous activity arises from certain underlying physiological origins. We shall attempt to show from studies chiefly on the white rat what some of these origins are, and how they fit into the general biological picture of the animal's life.

The investigations described below have been made by Ging H. Wang, Elaine F. Kinder, Tomi Wada, and the present writer in Psychobiological Laboratory of the Phipps Psychiatric Clinic during the past six years. Some of the experiments have already been reported elsewhere, but we have taken this opportunity to collect also numerous observations that are as yet unpublished.

PERIODIC NATURE OF SPONTANEOUS ACTIVITY—TWO-HOUR RHYTHM

We may begin our review with the rat confined just after feeding in a small cage. The walls and floor of the cage are absolutely bare and the room in which the experiment is performed is kept constantly illuminated and free from all disquieting noises and odors. If we observe the animal for a while we see that it moves about most of the time, doing many things. It sniffs and claws at the walls of the cage, it climbs, and gnaws and scratches; but from all these observations, however interesting at first sight, we learn nothing of what makes it active. If, however, we arrange the cage so that every movement therein, even the slightest, is recorded over a period of ten to twelve hours on a smoked drum, a remarkable fact comes to light: this diffuse gross bodily activity occurs rhythmically, active periods alternating with periods of almost complete quiescence. The active periods occur at intervals varying from one to two hours. Moreover, the records show further that the activity within each active period is slight at the beginning, but increases as the period advances and reaches its maximum usually near the end (Richter, 1922).

Had the motility been irregular and non-periodic we should have accomplished nothing in our investigation of its origin. On the contrary, however, it is very significant that such well defined periods of activity, recurring at such regular intervals, are found when external conditions are as nearly constant as is practically possible. This fact would indicate that the motility rhythm must be set up from within the animal in some organ which functions at a similar frequency of one and a half to two hours.

ORIGIN OF THE TWO-HOUR ACTIVITY RHYTHM

A review of the periodicity of the different viscera eliminates at once the heart and lungs and the sex glands, since the two former organs function at a frequency much higher and the latter at a frequency much lower than that of the bodily activity rhythm. In the stomach, however, we know that active periods alternate with quiescent intervals every hour and a half to two hours. In 1904 Boldireff was able to show that contractions occur in the walls of the empty stomach, but several years elapsed before Carlson (1916) demonstrated the periodic nature of this activity. Carlson, working with guinea pigs, dogs, and monkeys, found that from one to two hours after a meal, when the stomach is nearly empty, contraction waves begin passing downward over the stomach walls. These waves, small at first, gradually become larger and larger until they finally involve the whole lower half of the organ, and the gastric musculature often passes into a condition of semi-tetanus. Then, quite suddenly, the contractions cease and an inactive interval of an hour or more ensues. After this period of quiescence the small waves begin again, and the entire process is repeated. Thus one period follows another as long as the stomach remains empty.

Gastric movements in both animals and man have been studied by a balloon attached to the end of a tube, passed through the œsophagus into the stomach and inflated, and a manometer is fastened to the other end of the tube. The contractions of the stomach change the pressure on the balloon, so that some of the air is pushed up into the tube, and the level of the water in the manometer is changed. As the water rises and falls with the contraction waves, the movement is recorded by a floating pointer on a smoked drum.

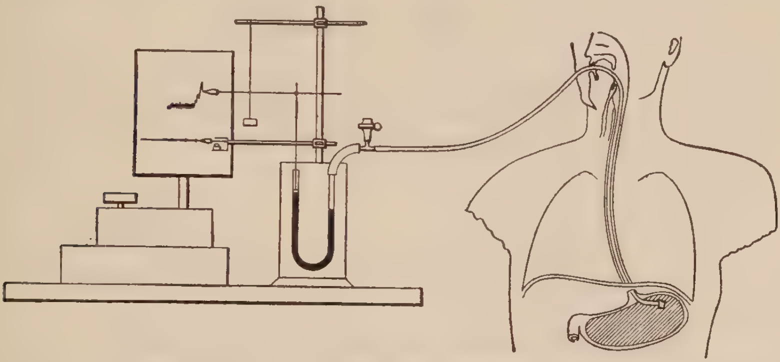

FIGURE 7 (original FIG. 4). DIAGRAM SHOWING THE METHOD OF RECORDING THE HUNGER CONTRACTIONS OF THE EMPTY STOMACH
(After Rogers and Martin, 1927)

According to the recent observations of Rogers and Martin (1926), the stomach takes one shape at the height of each of the single contractions near the end of the active period and another when it is inactive and relaxed. With each of these large waves, then, we see that the lower part of the stomach contracts to such an extent that the lumen practically disappears, whereas the upper part may show a contraction wave near the middle.

Cannon and Washburn (1912) and Cannon (1915) have shown that with these large contractions the sensation of hunger arises, and from the work of Rogers and Martin we know that these "hunger" contractions are set up in the lower third of the stomach. The hunger sensation is not produced until the end of the active period is nearly reached, when the waves have become very large, but even then it increases in intensity to some extent with the magnitude of the contraction, and it disappears entirely when the contractions cease. The inference, therefore, is quite logical that the two-hour periods of gross bodily activity in the rat are associated with periods of gastric movement and have to do with the hunger responses of the animal.

CORRELATION BETWEEN THE TWO-HOUR ACTIVITY RHYTHM AND THE HUNGER RESPONSE

In order to test this hypothesis we recorded simultaneously the bodily activity of the animal and the intervals at which it sought food and ate. The larger compartment was a simple cage; the smaller compartment contained a cup filled with a powdered food mixture (McCollum diet). The corners of each cage were supported on rubber tambours and so arranged that the activity in the two cages was recorded separately. The animal always enters the feeding-cage and eats once during each activity period. It enters usually near the end of the period and rarely at the beginning, and it does not enter during a quiescent interval. Moreover, we have found through prolonged personal observation, that an adult animal rarely approaches the food-box *except to eat,* and that the vibrations recorded in the activity cage for a short time after it leaves the food-box are produced almost entirely by an extensive cleansing performance which always follows feeding. In these experiments the activity periods, recurring invariably with the entrance into the food-box, are even more regular than they were in the earlier work when no food was available. With this greater regularity, the quiescent intervals are much longer, so that the lapse between periods is three to four hours instead of one or two, as found

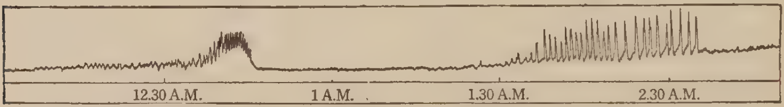

FIGURE 8 (original FIG. 5). RECORD OF STOMACH CONTRACTIONS OF A HUMAN ADULT TAKEN DURING AN UNINTERRUPTED SLEEP
(Drawing approximating the original photograph)

in the activity cage with no food-box attached. The significance of the lengthened intervals will be discussed below.

We may now attempt to show in more detail how the gross bodily activity, the feeding habits, and the stomach contractions seem to be correlated. The simultaneous records of activity and feeding suggest that a close relationship must exist between the periods observed in the simple activity cage and the hunger "drive" of the animal. From the experimental data compiled above we know that the motility rhythm and the stomach contraction rhythm have three features in common: the frequency of both varies between one and two hours; the active phases of both begin at slight intensity and increase gradually, reaching a maximum near the end; and in both the active period ends abruptly and is followed by a quiescent interval. If we represent the two rhythms as in A, Figure 9, drawing a diagrammatic activity record directly above a diagrammatic record of gastric movement, so that the active phases of the two coincide, we find that the small stomach contractions occur simultaneously with

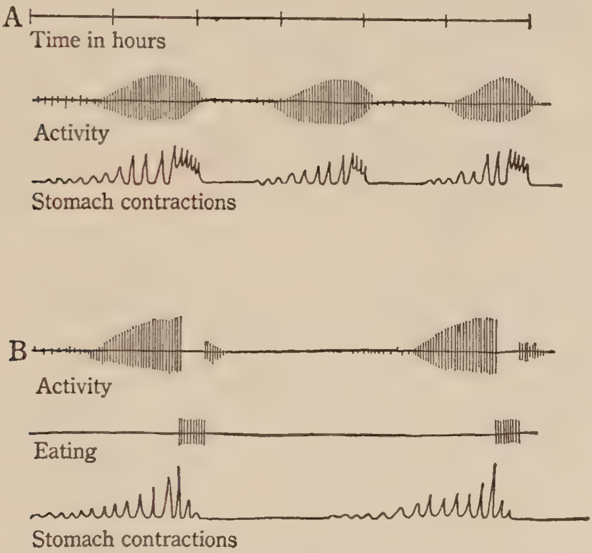

FIGURE 9 (original FIG. 9). SCHEMATIC REPRESENTATION OF THE RELATION BETWEEN PERIODS OF GROSS BODILY ACTIVITY AND STOMACH CONTRACTIONS

A. Simple activity cage without food. *B*. Double cage with food the beginning of the motility period, and that as the magnitude of the contractions increases the animal becomes more and more active. But how can we justify our representation in terms of the hunger response? It is very probable that as long as the animal experiences the hunger sensation which accompanies the stomach movements, it seeks for food, unsuccessfully, of course, in the single cage without a food-box. When the gastric contractions stop, however, the hunger disappears and the animal becomes quiet again.

On this basis one might expect that when food was available all the time, the rat would enter the food-box as soon as the contractions began. Actually, however, we know that it moves about in the main cage for some time before it approaches the food-box. How, then, can we explain this preliminary diffuse activity? Here again, as in A, Figure 9, the relationship may be represented schematically. In B, Figure 9, a record taken simultaneously from the activity cage and the food-box is shown in diagram on the first and second lines, and a stomach contraction record on the third. Thus we see that the small contractions give rise to the diffuse activity in the large cage. The animal seems at first simply to be annoyed and becomes more and more restless as the contractions grow larger, until the "main" contractions set in and the general discomfort becomes centralized in the hunger sensation. This stimulus dominates the behavior of the organism and it enters the food-box to eat. When its appetite has been satisfied, it passes into a period of quiescence which lasts until the stomach has become empty and the contractions have started up again.

We sought our relationship next in experiments on the human infant. Wada (1922) found that in a child ten months old, during a continuous uninterrupted sleep lasting eight hours, the activity, as recorded by a tambour and spring placed beneath the crib, was definitely periodic, the interval between the periods averaging forty-five minutes. This seemed to offer us an excellent opportunity, especially since Carlson and Ginsburg (1915) had found that the stomach tube could be passed quite easily into infants. Our plan was to take simultaneous records of the stomach contractions and the activity periods during sleep. In six babies used for these experiments we succeeded in passing the tube without too much difficulty, but then either the babies would not go to sleep, or else the tube was completely blocked by strong spasms of the cardiac sphincter. After many unsuccessful attempts this method of attack had to be abandoned. Meanwhile, Wada, taking records on medical students while they slept, succeeded in obtaining some very conclusive results. The students swallowed the stomach tube at ten o'clock in the evening, just before retiring, and simultaneous records were taken throughout the night of the stomach contractions and bodily movements. Besides diffuse activities such as turning over, smaller movements were recorded whenever possible. Adult activity during sleep, just as infant activity, proved to be periodic, but the interval between the periods is much longer, varying between two and three hours. The stomach contractions, of course, are also rhythmical, much more so than they are during the waking state. And the periods of these two phenomena coincide very well; when the stomach is quiescent the gross bodily activity is reduced to a minimum, but during the contraction periods frequent movements occur, the largest coinciding with the "main" hunger contractions. The record in Figure 10 illustrating this fact is similar to those obtained by Wada. The body movements indicated on the top line in this figure were recorded by means of a tambour and spring attached from beneath to the spring of the bed. Since only large movements such as turning over are registered in this way, it can be seen that just before the "main" hunger contractions set in, the subject became very active, and again almost simultaneously with the largest contraction wave there was an even greater amount of activity.

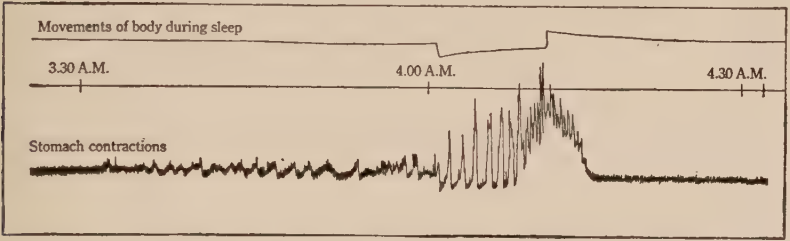

FIGURE 10 (original FIG. 11). SIMULTANEOUS RECORD OF GROSS BODILY MOVEMENT AND STOMACH CONTRACTIONS OF A HUMAN ADULT TAKEN DURING DEEP SLEEP

The results of most of our supplementary experiments, therefore, while not conclusive, would seem to uphold our theory that the two-hour activity rhythm in the rat is dependent on gastric function.

FEEDING HABITS OF THE RAT

Starting from our observations on the relation between activity and the feeding periods of the rat, we decided to extend our investigation to a study of its food habits, with special reference to their regularity. For this purpose cages were constructed, consisting of individual compartments large enough for the animals to take plenty of exercise, with a small tunnel on one side at the top leading to the food-box. This tunnel was built in an inconvenient position in order to discourage the rat as much as possible from entering it except when driven by hunger. The food-cup was placed at the end of the tunnel under the wire cloth floor, and a hole was made in the floor just large enough for the rat to insert its head. Whenever it reached in for food, the balance of the cup on a large tambour was disturbed and a mark was made on a smoked drum.

The records obtained in these cages bring out clearly the great constancy of the feeding habits of the rat. Similar experiments on other animals confirmed these results. A rat may eat seven times a day or it may eat eight or ten times, but in any case it maintains a constant average from one day to the next.

From the above experiments it seems fairly well established, then, that the hunger contractions stimulate the organism to activity, but what the details of the mechanism are we are not prepared to discuss. We do believe, however, that somehow, with each new hunger contraction period, impulses are sent up the afferent nerves from the stomach to the brain and out to the striped muscles, to release the energy stored up there.

GENESIS AND DEVELOPMENT OF THE FEEDING HABITS

But how does the activity produced in this way become associated finally with the eating process? We cannot assume that the new-born animal seeks food when the contractions begin. Indeed it would seem more probable that the relationship is built up by the usual trial and error method. In order to solve this problem one must learn more about the activity of the very young animal. Does it show the periodic motility of the adult individual?

The activity of the new-born rat was recorded in certain cages. The bottom of the cage consisted of a wooden frame 6 inches square with a sheet of rubber dam stretched taut across it.

The rubber was covered with pieces of flannel and the sides of the cage, made of paper, were pasted on the wooden frame. Every movement of the animal in the cage was transmitted through the rubber membrane to a lever which recorded on a smoked paper drum. The young rats were left in the cages for twelve hours at a time, and then returned to their mothers for twelve hours.

Records obtained in this way showed that the motility of the rat immediately after birth is continuous rather than periodic. For the first ten days it remains constant; then a rhythm begins to appear, and by the sixteenth day clear-cut and very regular intervals are present. This result is consistent with the fact that the new-born of some animal species show stomach contractions with almost no indication of periodicity (Patterson, 1914). On the basis of this knowledge, then, we may picture how the diffuse activity of the new-born rat resolves itself into a search for food.

In its almost continuous motility, the very young animal sucks at everything with which its mouth comes into contact,—the feet and legs of its litter mates, the straw of the nest, hair on its mother's body, and, eventually, the mother's teats. When it sucks at anything other than the teats, nothing results; the stomach contractions persist and the activity continues as before. When it sucks at the teats, on the other hand, an entirely different situation arises; milk fills the stomach, the contractions cease, and the animal grows quiet. Then as the stomach becomes empty again, the process will be repeated. The animal may happen upon the teats at the very beginning, or it may reach them after a prolonged active interval. In either event, the excursion always ends with the feeding process. The young doubtless frequently find the teats through the intervention of the mother, but largely by the trial and error method there is gradually established, on a conditioned reflex basis, an association between discomfort due to stomach contractions, feeding, and subsequent relief. In the adult rat, the preliminary restlessness which occurs during the time of small contractions is diverted at once into specific food seeking activities as soon as the main contractions begin. [This interpretation is not in harmony with the organismic point of view. Ed.]

It is interesting in this connection that the new-born guinea pig, rabbit, kitten, and chick, unlike the rat, show periodic activity at birth. But how may this fact be brought into relation with the view developed above? It is important to note that while the rat is still in a comparatively embryonic condition at birth, these other animals are all fairly well developed and co-ordinated. The rat, for several days after birth, progresses much as a worm does, crawling and wriggling its way about; the new-born guinea pig, on the other hand, actually walks almost at once. In keeping with their periodic bodily activity, the more highly developed individuals probably show periodic stomach contractions at birth. We have as yet done no experiments to test this theory, but we hope soon to observe in detail the feeding habits of these animals to determine how the habits differ from those of the rat with regard to their genesis and development.

FURTHER OBSERVATIONS ON THE PERIODIC NATURE OF SPONTANEOUS ACTIVITY. FOUR-DAY ACTIVITY RHYTHM IN THE FEMALE

So much for the three- to four-hour activity rhythm apparent in records taken during twelve to twenty-four hour periods. Our observations must now be extended over days and weeks instead of hours. The small triangular cages are impractical for this purpose because of the length of time required to count the individual marks on the smoked drum record. Activity for long periods can be measured more simply and much more accurately in the type of cage which consists of a small living compartment just large enough to accommodate a food-cup and an adult animal, and a revolving drum to which the animal has free access at all times. By means of a cyclometer connected with the axle of the drum by an excentric lever, all revolutions, clockwise and counter clockwise, are recorded. The rat, like many other small rodents, seems to enjoy running and spends much of its time in the drum. Although the daily activity averages between five and ten miles for most of the animals, as many as twenty-seven miles have been recorded for one individual in twenty-four hours.

Here again, casual observation of the running activity discloses nothing of its origin. Continuous records over long periods of time, however, reveal a fact far more striking than the tri-hourly rhythm described above. In the female rat the activity falls into a regular four-day cycle, most females running eight to ten miles every fourth day and but a fraction of a mile on the three days intervening. Since this rhythm, which was described independently by Wang (1923) and Slonaker (1924), occurs in an environment free from any cyclic disturbance, it, too, must have its origin within the organism. But what organ functions in the rat at a four-day rhythm?

FOUR-DAY ACTIVITY RHYTHM AND THE OVULATION CYCLE

Observations of the ovulation cycle of the rat made by Long and Evans (1922) answer this question at once. These workers have shown, by the methods of Stockard and Papanicolaou (1917), that the length of the œstrous cycle in the rat is four days, with individual variations above and below this average. As is well known, this was determined by histological studies of cast-off cells scraped with a small spatula from the vaginal mucosa. During the diœstrum nucleated epithelial cells and leucocytes are present, whereas during the periods of œstrus and ovulation only cornified cells appear.

Wang and Slonaker have been able to show by means of simultaneous smear and activity records, that the peak of running activity every fourth day just precedes ovulation. This is undoubtedly one of the most interesting correlations that have been made in animal behavior.

The question arises then, as to the relation between the running activity and the sex "drive" of the animal. Wang has found that females will mate for a short time just before and just after the peak of running activity is reached, while at all other times they are completely indifferent or even averse to the male. The exact relationship between running activity, vaginal smears, and sex activity, have been worked out by Wang. Records taken every six hours on all three phenomena showed that the great burst in running activity recorded on the day of œstrus is confined almost entirely to the six-hour interval which immediately precedes the appearance of cornified cells in the vaginal smear.

Obviously, therefore, the spontaneous activity is dependent on ovarian function. The degree of dependence can be demonstrated directly by numerous experiments performed on animals in which the ovaries were not functioning. Prepubescence, senility, pregnancy, pseudo-pregnancy, lactation, and castration all show a clear-cut effect in the activity readings. In Figure 11, a typical normal record is presented to show the low running level before puberty and the sudden pubescent burst.

Pregnancy causes a 60 to 95 per cent. decrease in activity which lasts through the entire gestation and lactation period (Slonaker, 1925; Wang, 1923). Pseudo-pregnancy, produced when the tip of the uterus is stimulated with a glass rod introduced through the vagina, results in an immediate decrease which persists for fifteen days (Wang, 1923), and sterile copulation performed by a vasectomized male has the same effect (Slonaker, 1925).

Similar results were obtained in a series of experiments in which a normal female had constant access to a cage containing a vasectomized male. In these experiments a sex-box attached to the usual living-cage connected with the revolving drum, was so arranged that the female could pass freely back and forth to visit the male, but the male could not get into the living-cage and revolving drum. This separation was achieved quite simply by means of a board partition with a hole just large enough to admit the female, but too small for the male. In order that we might differentiate running activity changes caused by the extra compartment from those caused by the presence of the male, we attached the sex-box a week before the male was introduced. The record shows that the mere addition of the sex-cage produced no effect. When the male was placed in it, however, there was an immediate and prolonged decrease in activity which persisted for eight to ten days after he was removed again.

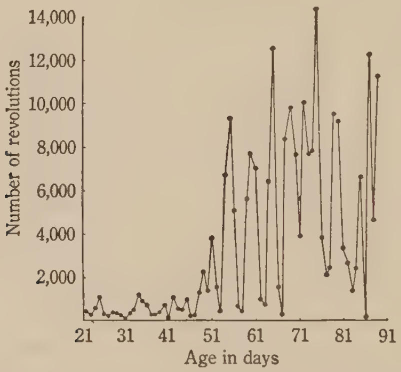

Figure 11 (original Fig. 22). Record Showing the Burst of Activity and Appearance of the Four-day Cycle at the Time of Puberty (After Wang)

Figure 12 shows the effect of complete removal of the ovaries: the activity drops (60 to 95 per cent.) to a flat, low level and the four-day cycle disappears completely. When an animal is spayed before puberty its activity always remains low and non-rhythmical (Wang, 1923; Slonaker, 1924).

Finally the dependence of the running activity on the ovaries may be demonstrated most strikingly by the effects of ovarian implantation in spayed animals. Whenever the grafts "take," the activity begins to increase almost immediately, and it grows gradually higher until the normal running level of the female is reached. Then, if the grafted ovary is removed, the activity drops about 60 to 95 per cent., just as it does in the normal female after spaying.

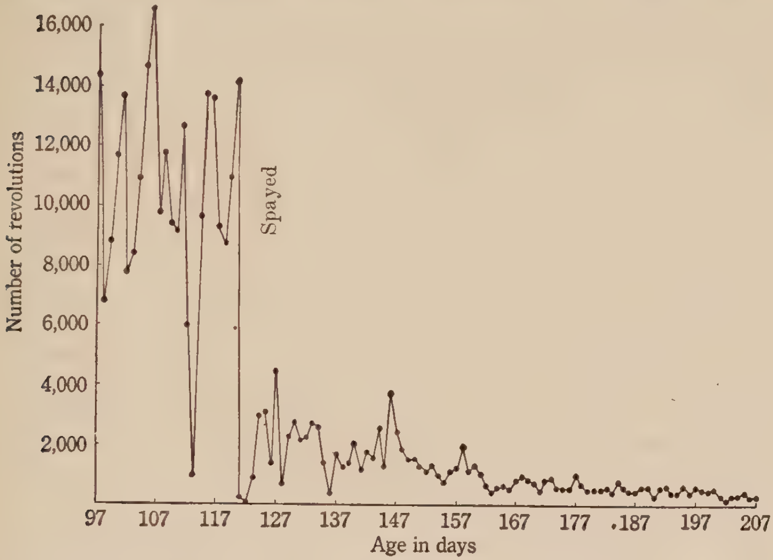

FIGURE 12 (original FIG. 26). RECORD SHOWING THE EFFECT OF THE COMPLETE REMOVAL OF BOTH OVARIES

These experiments show definitely that the high running level is dependent upon some substance secreted by the ovaries into the blood stream. The question now arises as to what part of the ovaries produces this substance. Bugbee and Simond (1926) have thrown some light on this problem by experiments in which the extract from pigs' follicles was injected into spayed rats. Such a procedure produces the same effect that successful implantation of an ovary would have produced,—the activity shows a marked increase from the low spayed level to the level of normal animals and the genital tract resumes its normal condition.

Accordingly he has concluded that there may be in the ovarian secretion one specific substance for furthering the growth and development of the genital tract and another for the production of activity. And on the basis of experiments in which he traumatized the ovaries (Wang and Guttmacher, 1927), this suggestion becomes even more plausible. It was found quite by chance that a small remnant of ovary left in the body produces very striking changes in both smear and activity findings. The activity, except for a drop of short duration immediately following the operation, usually regains its original level, but the four-day cycle is entirely absent. Coincident with the high irrhythmic activity level, the vaginal smears usually show cornified cells, the indication of œstrum in the normal animal. In some individuals, however, the other types of cells, as well as cornified cells, appear and disappear with very little relation to the changes in activity.

The question as to how and where the secretion acts to produce the activity has not yet been answered. We thought at first that, just as the three- to four-hour rhythm was set up by stomach contractions, so the four-day cycle might have its origin in the contraction of some similar hollow viscus in the sex-apparatus, probably the uterus. Contrary to our expectations, however, Wang found that removal of the uterus affects neither the level of activity nor the four-day cycle, and Hoskins (1925, II) has recently confirmed these observations. The secretion, then, must take effect in some part of the central nervous system, either by simply increasing the irritability of the centers in the brain and spinal cord, or by actually stimulating those centers. Although definite evidence is lacking at present, we are inclined to hold to the latter possibility.

Spontaneous Running Activity of the Male

In the male the spontaneous running activity does not show the four-day cycle of the female, and the general average is somewhat lower. However, it is dependent on the sex glands, for when the animal is castrated the activity drops about 60 per cent. to the low level of the spayed female (Fig. 13). Hoskins (1925, IV) was able to demonstrate no change in activity when testes were transplanted to the castrate, but we have found that the activity immediately increases to the normal running level (Richter and Wislocki, 1927). And transplantation of ovaries brings about an even greater increase (Wang, Richter, and Guttmacher, 1925). The activity then reaches the high level of the female, and, what is more interesting, it also shows the four-day rhythm. When the grafts are removed the castrate effects appear again.

That the relation of running activity to the sex "drive" is not so clear-cut in the male as it is in the female, is demonstrated in a series of experiments in which each male was removed from the running cage for a half hour each day and placed with a female in "heat" in a large stock cage. We found in this way that males showing a low running activity took no interest in the females and made no effort to copulate, whereas males with a high running activity copulated frequently. Contrary to the effect on the females, however, copulation, even when repeated as many as thirty times, produced no noticeable change in the running level of the male on the following day.

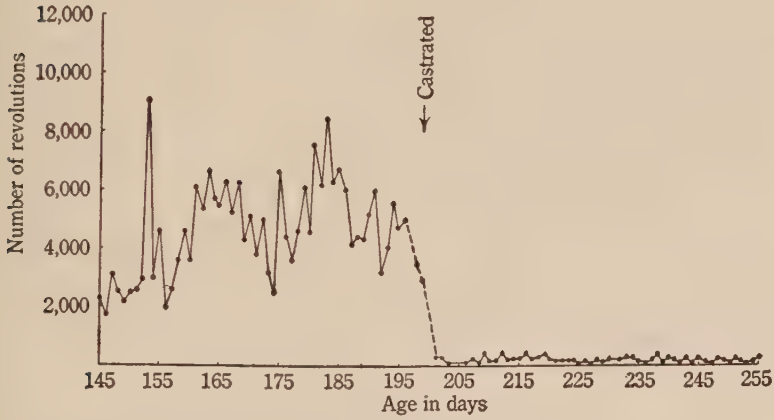

FIGURE 13 (original FIG. 29). SPONTANEOUS RUNNING ACTIVITY OF A MALE, SHOWING THE EFFECT OF CASTRATION

Nothing is known regarding the mechanism involved in the production of the activity of the male. That the removal of the seminal vesicles produced a no more noticeable effect than did the removal of the uterus in the female can be shown. We are trying to discover on what part of the testes the activity is dependent by a method similar to that used by Stockard and Papanicolaou in the female. We have been extirpating the testes in some animals at a peak of activity and in others at a depression with the expectation that detailed histological studies of these organs may disclose what parts are responsible for the fluctuations in activity. Although we have obtained no conclusive data the approach is promising.

It must be borne in mind before we pass on to a discussion of other rhythms that the work reported above has simply demonstrated to what extent the overt bodily activity *is dependent* on the sex hormone. None of our experiments has shown that the hormone actually *produces* the activity. Very probably a number of other organs are equally important, so that the elimination of any one of them would be sufficient to bring about the large decrease in activity which follows spaying or castration. We have tried removing the thyroid, parathyroid, and the pituitary, but in so far as our operations were successful, they produced no change in activity. Removal of the adrenals, on the other hand, causes a decrease in the running average, although the sex cycle still remains. It can be seen, then, that we still do not have complete knowledge of the mechanism which determines the composite picture of the spontaneous activity.

Other Activity Rhythms

With a knowledge of the cyclic fluctuations in activity we may learn a great deal about changes which occur in the different organs within the body, when outwardly or without sacrifice of many animals nothing could be learned. The discovery of the four-day activity rhythm in the female rat, for instance, would have led very quickly to the discovery of the œstrous rhythm had it not been already detected by other means. This same principle may be applied in a search for cyclic changes in other internal organs, since we have found periods of activity longer than four days in both males and females after the sex organs have been removed. There are at least two other fairly well-defined rhythms, one of from seven to ten days, and one of from sixteen to thirty days. Besides these two rhythms a number of others varying between forty and one hundred and twenty days have occasionally been observed by Slonaker (1926) and by us.

The question arises now as to the origin and significance of these rhythms. The other internal glands, the thyroid, the parathyroid, the pituitary and the adrenals, suggest a possible source. We have been removing each of these organs at various phases of activity but we have been unable as yet to make a study of the histological sections.

So much for the origin of these rhythms. What is their significance? Are they associated with some specific performance just as the three- to four-hour and the four-day cycles are? It may be that, coinciding with these slower fluctuations of running activity, changes in such pursuits as burrowing, climbing, gnawing, fighting, nest-building, and other specific activities can be demonstrated. However, before we take up the question of these more complicated behavior patterns, we must consider briefly some of the purely physiological mechanisms in order that we may comprehend the complete activity picture presented by the organism.

OVERT RESPONSES OF A PURELY PHYSIOLOGICAL NATURE

For this reason we have studied drinking, urination, and defecation, to determine whether these functions, too, are periodic. Records of the time interval at which these three responses occur show that they are all rhythmical.

Thirst was studied by means of the specially constructed cages. A recess large enough for the animal to enter and drink, but too small for it to lie down or turn around, was built on one side of the cage and an inverted watering tube was placed at the end away from the cage. The bottom of this recess was made of a piece of aluminum pivoted at one end and supported on a tambour at the other, so that whenever the animal entered to drink a mark was recorded on a smoked drum. The records show that the rat drinks about ten times a day at intervals of two and a quarter hours. The periodicity of the thirst response is quite as remarkable as that of the hunger response, in view of the current conception that the rats eat and drink at very frequent and irregular intervals.

Urination and defecation were also recorded. The small cage with a ½-inch mesh wire-cloth bottom, rested over an 8-inch paraffined tin funnel. When the rat defecated, the feces slipped through the wire floor into the funnel and on to an inclined trough of fine-meshed wire below. As they dropped from the end of this trough they struck a small paper disc fastened to the end of a tambour, and the process was recorded. Urine was also collected in the funnel but it passed directly through the wire trough to another paper disc attached to another tambour.

From simultaneous records of urination and defecation obtained by this method, it was found that although these two functions were very regular, they are quite independent. Urination occurs approximately every two hours; defecation, every five hours, but the group of animals studied up to the present time is not sufficiently large to permit accurate statements of the time interval, nor can we show how these rhythms vary with age and sex.

These phenomena must reveal the periodicity of the bladder and rectum, but so far we do not know in what origin thirst is localized. Possibly through our knowledge of its periodicity we may obtain some clue as to its origin.

The results of all of the experiments discussed above have shown how largely the spontaneous activities of the rat are periodic in nature, and are associated with periodically functioning organs. From these results, we see the animal as an organism carrying within itself various mechanisms discharging at different rates, to a great extent independently of one another. By way of summary we have listed in Table I the different activities according to their periodicity and the organ with which they are associated.

TABLE I

Activity	*Periodicity*	*Associated Organ*
Urination	2–3 hour	Bladder
Drinking	2–3 hour	?
Defecation	3–5 hour	Rectum
Eating	3–4 hour	Stomach
Mating ♀	4 day	Ovaries
Nest-building	?	?
Gnawing	?	?
Burrowing	?	?
Fighting	?	?
Migrating	?	?
?	7 days	?
?	18–22 days	?
?	40–120 days	?
?	?	Adrenal
?	?	Pituitary
?	?	Thyroid
?	?	Parathyroid

CONSTRUCTIVE ACTIVITIES

With this work on the simpler forms of spontaneous behavior as a basis, we may now single out for study the more complicated and specific performances of the rat, such as burrowing, gnawing, nest-building, and social activities. At the present time nest-building is the only constructive activity which has been quantitatively studied. We may ask, then, what it is that makes the rat build nests. In order to answer this question we must arrange our experiment so that nest-building is practically the only outlet for the animal, and all other activities are either kept constant or eliminated. Such a situation in which spontaneous nest-building can be measured has been worked out very satisfactorily by Kinder (1927), in two different types of cages.

In these cages the activity is measured in terms of the number of strips of crêpe paper used each day by the rat in building its nest. The method of presenting the paper differed in the two types of cages. In the first type 600 strips, ½ inch wide and 6 inches long, were scattered each day evenly over the floor, and the animals built their nests by pushing the strips into heaps. In cages of the second type, 250 strips, 12 inches long, were hung over the sides so that the animal had to pull down one strip at a time and take it to the nest. With these cages Kinder found that if the nests are removed each day rats of all ages will build a fresh nest within the following twenty-four hours. That the activity is present and equally strong in both sexes is shown by records of the daily nest-building activity. Moreover, nest-building is practically independent of experience, since young rats thirty days old raised in sawdust build perfect nests out of the crêpe paper the first time it is presented to them. But what is it that drives the rat to build nests? Again observations for short intervals tell us nothing about the mechanism involved. Observed over long periods of time under controlled conditions, will it also be periodic like the running activity? Here Kinder found that the four-day rhythm of the female is present just as it was in the running activity, but it has a very different relation to the cycle. Nest-building is greatest in the diœstrous interval of low running activity and lowest during œstrus when the running activity is highest. At parturition and during lactation the nests are very large, but the controlling mechanism is obviously not an essential part of the reproductive activity since the males often built nests as large as those of the mother rats.

Kinder found that all phases of the nest-building phenomenon have one feature in common that indicates the origin of the activity: every phase can be understood as a part of the heat-regulating mechanism by means of which the body-temperature is maintained at a constant level. The activity increases in low temperatures and decreases in high temperatures. It is high when there is a tendency for the body temperature to decrease, before puberty, during the inactive diœstrous interval, during pregnancy and lactation, and during starvation; it is low at œstrus, when the animal is very active and the body temperature tends to increase.

We may regard nest-building as a part of the heat-regulating mechanism, just as we consider the growth of fat and hair during cold weather a part of the more primitive physiological manifestations of heat conservation. Nest-building, like the building of shelters and the wearing of clothing, is a much more highly developed method of maintaining a normal body temperature, but it is nevertheless an expression of the same impetus that produces the increase in fat and hair in more primitive animals (Martin, C. J., 1901). The mechanism of the "drive" involved here, therefore, is very different from that present in either the hunger or the sex activities.

Allied to nest-building in the rat are probably burrowing, the tendency of the animal to wedge itself into small spaces with contact on all sides, and its desire to huddle together with several of its mates. These activities also tend to conserve heat, in contrast to running, climbing, and jumping, which contribute to the maintenance of a constant body-temperature through heat production.

The various performances which, although associated with other drives, may be considered as contributing to the heat-regulating process, can be grouped as follows:

Nest-building	conserve body heat
Burrowing	
Huddling into small spaces	
Huddling together—social contact	

Running	increase heat production
Climbing	
Jumping	

Thus far we have investigated the different activities of the rat separately under more or less isolated and controlled conditions. We believe that it is possible, however, to study the animal under normal conditions, when it can indulge in any of the activities present in its usual outdoor environment. For this purpose we have constructed a set of cages in which a record can be obtained of the time spent each day by the rat in climbing, running, burrowing, gnawing, eating, drinking, and mating. This cage makes it possible to account for the activity of the animal each day during every minute of the twenty-four hours. The large triangular central cage and all of the smaller cages at the side, except the revolving drum and the climbing tower, are supported on tambours. Running activity is recorded both graphically and with a cyclometer; climbing activity, by counting the number of times the animal goes up and down the tower each day. Both the entrance to the tower and the top part are supported on tambours, so that a record is made on smoked paper each time the animal starts up the tower and when it reaches the top.

Because of the fact that we had only one cage we were not able to gather enough data to be of statistical value, but we have made sufficient observations to know that with some refinement of this method the behavior of the rat can be thoroughly studied in the laboratory. Certainly with this type of cage we can obtain a normal environment for the animal, in so far as it has an outlet for most of its different drives, and if we can judge how normal the environment is by the type of rat that it produces, the success of the method is unquestionable. An individual brought up in the middle cage is far more intelligent than one raised in the ordinary running cage. Within the multiple cage the animal shows all kinds of constructive and imaginative activities, rarely, if ever, seen in an ordinary laboratory rat, and on occasions when it escapes it avoids recapture with extraordinary success. We may note in passing that were rats of this environment used for experiments on extirpation of the different areas in the cortex of the brain, much more noticeable defects in behavior might possibly be demonstrated as a result of the injuries than have appeared thus far in the rat kept under the usual laboratory conditions.

Individual differences are quite striking in the multiple cage if we may judge from our small number of animals. One rat will spend most of its time in the climbing tower, passing up and down as many as thirty or forty times a day; another will spend all of its time in the sex-box; and another will gnaw all day long. None of our records have been taken over sufficiently long periods to permit us to give any perspective on either the constancy and fluctuations or the importance of the different activities in the life of the rat, but we do know that in this cage the running activity in the drum is reduced in every case to a few hundred revolutions per day. It is very interesting also, that in these cages where the animal has many different diversions the frequency of its eating period is greatly reduced. It enters the food-box once every five or six hours, and sometimes even less frequently, but whether it actually eats less food we have not yet determined.

That even the most complicated form of the rat's behavior may be studied in this cage is brought out by the following observations. In experiments on one animal a liberal supply of building material,—sticks, rope, stones, and cloth,—was placed in the large central cage. This animal had habitually deposited its feces in the water-cup. Usually the water was changed every day, but on one occasion, by some neglect, it was not changed for several days, so that the resulting odor became very unpleasant. At this point the animal started to cover the hole over the water-cup. It first removed part of the upper layer of the cardboard bottom of the large central cage, and dragged it into the water-box. It placed the cardboard over the cup and smoothed it down on all sides until the hole was perfectly covered. Then from the bottom of the central cage it lifted stones larger than its head three inches into the drinking cage and placed them over the cardboard cover. Besides the large stones numerous pebbles and sticks were used until the water-box was completely blocked. The animal had cut off its only water supply by this performance. Since we wished to see what it would do when it became very thirsty, the material was left undisturbed and no other water was given. After three days, the animal pushed all of the sticks and stones from the drinking cage into the large central cage, tore up the cardboard seal, and drank its fill of the polluted water. This observation is certainly comparable to those made in the field. Had our apparatus been working better just at this time we should have obtained a complete biological record of this very interesting incident.

Another performance likewise constructive but less complicated was frequently observed. The rats plug up the entrance to any of the smaller cages whenever they have been frightened on entering them. This is the only explanation that can be offered for the frequency with which adult rats closed the entrance to the running drum shortly after they were placed in the cage. In one instance the opening was plugged up so tightly with nest-building material that a knife had to be used to open it up again.

Many similar examples could be given to demonstrate that in our artificially constructed environment practically every variety of behavior observable in a natural environment is obtained. And with a record of the way in which the animal spends its time before and after such episodes much more light will be thrown on their origin.

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GLOSSARY

Castration, excision of male or female reproductive organs (testicles, ovaries); generally refers to the male.
Copulation, sexual connection or intercourse; coitus.
Cornified Cells, hard, horny cells.
Cyclometer, instrument for measuring number of revolutions.
Defecation, evacuation of the bowels.
Diœstrum, interval between periods of sexual heat.
Epithelial, pertaining to epithelium, the cells covering all cutaneous (skin) and mucous surfaces (together with the secreting cells of glands developed from the same tissue from which the skin and mucous linings develop).
Follicles, small secretory cavities or sacks.
Gastric, pertaining to the stomach.
Genital Tract, tract through genital organs.
Gestation, pregnancy.
Hormone, vital principle; energizing extract in the blood stream.
Hyperthyroid, showing the effects of an excessive thyroid secretion.
Lactation, the formation or secretion of milk; also means the time of suckling.
Leucocytes, white blood corpuscles.
Lumen, the cavity of a tubular structure.
Manometer, an instrument for measuring the pressure exerted by liquids and gases.
Nucleated, containing nuclei, the vital or controlling centers of living cells.
Œstrum, sexual "heat" of animals; period of sexual excitement; œstrus cycle, cycle from one period of "heat" to the next.
Ovulation, the maturation and escape of ova (egg) from the ovary.
Parturition, act of giving birth to young.
Pituitary, ductless gland at the base of the brain.
Prepubescence, before the age of ability to reproduce.

Rectum, lower part of the large intestine.

Senility, old age.

Smear, sample of secretion or other liquid or semi-liquid substance smeared on a slide for microscopic observation.

Spatula, a broad, flexible blade.

Spayed, ovaries or testicles removed (more commonly the ovaries).

Tambour, a drum-shaped instrument with flexible diaphragm on one side; air forced in and out of the chamber of the drum displaces the flexible diaphragm; a recording pointer poised on the diaphragm records the displacements; used in connection with the pneumograph and other pneumatic recording devices.

Thyroid, ductless gland in the neck.

Traumatized, shocked or injured.

Vaginal Mucosa, mucous linings of the vagina.

Vasectomized, *vas deferens* tied off or cut; the *vas deferens* is a tube leading from the testicles, through which the male reproductive cells (spermatozoa) reach the seminal vesicles (retaining reservoirs) in the abdominal cavity.

Viscus, any organ contained in the cranium, thorax, abdominal cavity or pelvis.

GROUP IV

LEARNING

READING FOURTEEN[1]

HOW TO DEVELOP AN INTEREST IN ONE'S TASKS AND WORK[2]

Editor's Note

The New versus the Old Concept of Learning.—At the beginning of these readings, human behavior was studied without systematically restricting its conditions. Accordingly, social features were its outstanding characteristics. When, for convenience, the conditions for an investigation of behavior were confined to the problem-character of situations and to the organism's level of maturation, a different aspect of behavior stood out, namely, its intelligent character, the fact of always being *organized* with respect to a *goal* although the situation had never been met before. It was found that the complexity of the situation to which the organism could respond perfectly the first time was determined by the organism's level of maturation. Again, when for convenience the conditions of behavior were confined to those that increased the organism's tensions through intra-organic stimulation, still another aspect stood out, its emotive character. A third step will now be taken in the restriction and control of conditions. Those modes of behavior will be studied whose chief characteristic depends upon *repetition of stimulus-situations.* Under these conditions the organism's behavior becomes more complex, that is, more highly differentiated, still retaining its configurational properties. The organism is under tension toward a goal, but, with the differentiation of behavior, the goal also becomes more complicated and at the same time more definite. When these processes take place, the behavior is known as *learning.*

The difference between the organismic interpretation of the learning process and the prevailing theories of learning is so great that the beginning student is likely to be confused when at first the two views are contrasted. This is partly caused by the fact that the organismic view seems, on the surface, to contradict common sense. It is common sense to assert that one learns *by* experience, but this is not strictly true, if organismic principles are correct. Experience and learning are synonymous and the one cannot be explained in terms of the other. For example, a child picks up a bee and is stung. Thereafter he leaves bees alone. Did he learn by experience? If the fact of thereafter leaving bees alone is what is meant by the phrase "learning by experience," the term learning refers to the *discovery* that was made about bees. The learning took place in the act of perceiving the pain in its relation to the bee, or in other words, in the process of making a new configurational response. In the language of an adult, the child's experience could be described thus, "Ouch, this thing hurts; I will not touch one again." The learning is accomplished in making an organized response to the total situation, or in other words, in the experience itself. The child does not learn to avoid bees *after* he has been stung, but *while* he is being stung. Therefore, the experience does not account for the learning; it *is* the learning, and it takes the form of making a discovery. In perceiving the relation of pain to the bee the child was doing something that he had never done before. He was exhibiting insight in a new and strange situation.

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[1] Parallels *The Science of Psychology,* Chapter IX.

[2] By William F. Book, Indiana University. From the *Journal of Educational Psychology,* Vol. 18, 1927, 1-10. By permission.

It is also common sense to say that the child learned by *associating* the pain with the bee and with the movements of withdrawing the hand. But in terms of organismic principles this is not true, because it is an attempt to explain the whole in terms of the parts. The pain, the bee and the movements were not three isolated, unrelated bits of experience "coming into consciousness" separately, requiring the formation of *bonds* between them before they could be organized into a unified whole. Yet this is exactly what is implied by the principle of *association.* The association theory demands that a large number of separate acts be performed before the learning takes place. The child must perceive pain, see the bee, and make the movements; he must associate the pain with the bee, the pain with the movements, and the movements with the bee! But he must do still more. He must associate the *position* of the bee with the position of his hand, for he must learn that the *bee-he-is-touching* hurts him. He must associate the bee with the place where the bee was found, say the window sill, for that, also, was part of his experience at the time. But he did not *experience* a bee stinging him on the cheek, hence, if experience is necessary for learning, he would not know, thereafter, that a bee on the cheek might hurt him. He did not experience the sting from a bee found in a flower, so he would not know that bees found in flowers might hurt him. If learning is to be explained by the formation of associations, nothing is learned until after the associations are formed. *The child would have to be stung all over again in every new situation, obviously an absurdity!* Since no two situations are exactly alike, he would never learn.

Suppose a person is learning to typewrite. According to the association theory he must associate the position of each key with the appropriate movements of the fingers, but this is not so simple as it sounds. In writing words where letters are following one another constantly in a different order, *the movements of going from one key to another are different, so that for each sequence of letters new associations must be formed.* One does not make the same finger movement going from *a* to *t* as he does in going from *m* to *t; f* to *t* is still a different movement, and so on. *Each new combination of letters would demand a new set of movements not experienced before, that is, not yet learned, and therefore, impossible of execution.* This is obviously not the case.

Thus it is evident that there is something wrong with the experience and association theory. The illusion that experience explains learning comes, first, from the fact that they are one and the same, and second, from the fact that the learner observes the progress he is making and attempts to account for his present degree of proficiency in terms of preceding, lesser degrees. He forgets that with each step in his progress he is doing something *new*, experiencing something he has never experienced before, which, of course, previous experience cannot account for. If these new increments of progress were subtracted from the learning process, there would be no learning left. Doing the same thing over and over, that is, repeating one's performances, is not learning and can never lead to learning.

There are many other facts and arguments against the association and experience theory, but they cannot be discussed here.[3] It suffices to say that the organismic conception leads to other conclusions quite as revolutionary as the statements just made. Growth or maturation, not repetition, becomes the basis of learning, unless repetition is considered in connection with the stimulus-situation instead of the performances carried out by the learner. Repeating an act does not, as such, involve learning. *It involves the opposite of learning, the total lack of progress.* Drill and practice in the customary senses of these terms do not account for progress. Learning is not to be explained as an elimination of trial and error procedures and a mechanical fixation of accidentally successful performances. Learning is a configurational response, involving insight, and insight grows with stimulation and maturation. And finally, motivation, including the pleasure of achieving success in a task and the displeasure of failure, does not explain learning in the way that was heretofore supposed. The same conditions that account for the motives account for the learning.

However, these theoretical considerations do not detract from the importance of much experimental work that has been done on the learning process. The importance of goals and interest has not been slighted in spite of the fact that association theories have been unable to explain how interest and goals could possibly function. The following article written by Professor Book in the spirit, if not in the terminology of configurational psychology, shows clearly the importance of goals in learning. Interest means interest *in a task* and in the means of accomplishing it; interest implies tension toward a goal. Motivation is an increase of that tension, but motivation is impossible without *insight.*

Prior to 1924, when the new teacher-training law in Indiana went into effect, educational psychology at Indiana University was an elective course taken by students who had had an introductory course in elementary psychology and who were preparing to teach. Since 1924 it has been a required course for every student who expects to teach or who is qualifying for any kind of teaching certificate. This requirement has brought about a marked change in the mental attitude of the students taking this course and in the interest with which they pursue the work.

At the beginning of the second semester this year (1925-26) the 147 students enrolled in this course in the writer's classes were asked to state in writing (1) their real purpose in taking this course; and (2) what they expected to get out of the course. Sixty-six per cent. of these students stated that they took it because "they had to," or because "it was required to get a license." Twelve per cent. stated that they were taking it because they liked psychology; 1 per cent. because they liked education. The character of the answers given by the 12 per cent. who stated that they took it to prepare to teach indicated that the dominant idea in their minds was the same as for the 66 per cent.; *i.e.*, they were taking the course because it was required, not because they were interested in the subject or wanted the work. Only 22 per cent. of the students enrolled were interested in the course when the work began.

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[3] *Cf. The Science of Psychology,* Chapters IX, X, and XI.

The answers to the second question only strengthened this interpretation of the general attitude of students towards the course which had been found in previous semesters to be a serious handicap to the instructor in making the work of the course truly helpful to the students. That is to say the students now taking this course are not interested in the course and enter the class with the wrong attitude towards the work. This makes it very difficult to get the quality and amount of work from the students taking the course that should be done, a condition that is in sharp contrast to the condition which maintained two years ago when an attitude of intense interest in the work was the rule and when it was easy to get first class work from practically every student enrolled in the course. Why this change in interest and attitude towards the work? And what may be done to meet such a situation when it arises? Or stated in another way, how may a student's attitude towards his tasks be made favorable and a genuine interest in his work developed?

Before attempting to answer these questions let us inquire what interest really is and how such wrong attitudes towards any school subject as the one described are usually acquired.

What Interest Really Is

Not every one, not even a psychologist, knows exactly what interest is or what is meant when we use this term. Interest has been defined as: "the impulse to attend"; as "the recognition of a thing that has been vitally connected with our experience before, a thing that is familiar or old"; or "as a natural tendency to act." One psychologist says "the root idea of the term seems to be that of being engaged, engrossed, or entirely taken up with some activity because of its recognized worth.'' Dewey states that it marks the annihilation of the distance between the person interested and the materials and results of his action. According to him, interest is the sign of this organic union between the subject possessing the interest and the objects or materials he works upon. Other writers have made interest synonymous with the feeling of satisfaction or pleasure that *always* accompanies spontaneous and successful attention. Such a feeling of pleasure may be said to be the sign in our consciousness which notifies us that we are in reality growing interested in a subject or task.

These definitions will help us to understand and to keep in mind what interest really is.

IMPORTANCE OF INTEREST OR EFFECT WHICH IT PRODUCES ON ONE'S ABILITY TO WORK

Such an interest in one's tasks is very important as every one knows and should be developed and carefully maintained while we study or do our other work. In fact, one of the most necessary conditions for obtaining human efficiency in any line of work is to cultivate such an interest in whatever we have to do; and if we do not feel such a natural interest in our work to take steps to develop it at once, for keen interest is one of the most important driving forces in life.

This may be illustrated in a number of ways. A stentor (primitive water animal), busily engaged in the pursuit of food or if reacting to a particular kind of stimulus, will not respond to another stimulus that under other conditions would elicit a definite response. At such a time its organism seems physiologically ''set'' towards a certain stimulus and towards a particular type of movement so that a stimulus which normally excites it to action has no effect upon its behavior.

The same thing has been shown by experiments on animals. A hungry cat or rat placed in a puzzle box or maze shows by its every movement a mental and physiological adjustment that usually persists until it succeeds in getting out of the box. A sleepy or well-fed cat will, on the other hand, be ''set'' less towards escape movements when confined in such a puzzle box or maze. In such instances a certain psycho-physical adjustment has been set up within the organism that helps to determine what responses the animal will make and which creates a sort of inner urge that drives it on to make a type of movement that is continued until the animal obtains an end that may be said to be desired only in a physiological way. Or if put in terms of attention, or interest, we may say that the cat is very much interested in getting out of the box and will usually stick to the attempt until she succeeds.

Something like this, only on a far bigger and more complicated scale, is what happens with students, only here a purpose, an idea, a desire, some mental attitude or general feeling tone serves as such an inner urge, and helps to determine what sort of responses the individual will make when confronted by his tasks. Anything he is intensely interested in or is ''set on doing,'' he usually finds a way to accomplish. If there is no way, he proceeds to make one.

This has recently been illustrated by a number of important experiments and incidents from practical life. Rose Fritz won the world's typewriting contest in 1906 by writing 82 correct words each minute for one hour. She told Mr. Kimball, the director of the contest, that she had reached the human limit for this type of work—that she had written as fast as any human being could ever learn to typewrite.

Mr. Kimball, however, told her she was wrong. He assured her that she had not reached the limit, and convinced her that her own record could be passed. The result was that in the following year she beat her own record by writing 87 correct words each minute for an hour; and the following year she raised her record to 95 words, while today the world's record is held by Albert Tangora who wrote, in October, 1924, at the rate of 147 correct words each minute for one hour.

A study by H. D. Kitson of the increased output of 40 hand compositors in the printing industry showed that when these workers were subjected to a particular wage incentive which interested them in doing more and better work, these seasoned printers who had been working at the trade on an average 10.3 years made an average gain in output of work of 78 per cent. in five months. One man who had worked at the trade 23 years increased his output 289 per cent.

The results obtained in our own study of five different types of learning made with 124 college students emphasized in a still more striking way the value of interest in learning and work. Our experiments were devised to determine the effect which interest in improvement produced upon the progress in learning made by these subjects. In each type of learning studied, the sections that were being interested in the gains they were making, by the conditions of the experiment, made more rapid progress than did the sections that could not because of the conditions imposed by the experiment become interested in their own advancement. Moreover when the conditions were reversed for the various stimulus and control sections, the same individuals who had been making rapid progress when they were directly interested in their advancement ceased suddenly to improve, while the sections that had been making only slight improvement began suddenly to progress at a much more rapid rate as soon as they were definitely interested by the conditions of the experiment in the gains they were making. In each of the five types of learning studied this interest in improvement not only increased the learner's rate of gain but materially improved the quality of his work.[4]

Similar results were obtained by Remmers and Knight in an experiment made on college students. Ten college freshmen who had been previously hazed by their fraternity for a week and allowed to sleep only two hours a night were given some tests in addition at 10 P.M. and their results compared with the scores made by an equal number of junior students of equal ability who were tested under the most favorable classroom conditions at 8 A.M. The object of the experiment was to determine how much difference it would make in performing successfully this kind of work when the performers were keenly interested in the work. The freshmen were told by the president of their fraternity that they must push themselves to the limit on these tests as the score they made would determine their final acceptance or rejection by the fraternity. The juniors, on the other hand, had no *special* interest in the task or in their final score but were told, the same as the freshmen, to do their best on every test.

Under the conditions of this experiment the *freshmen* made an average of 21 columns of addition for each five-minute period of work in all the tests while the juniors made an average of only 11 columns per test. In other words the interest or special motivation which these freshmen students had, not only offsets the extreme fatigue produced by the hazing and the loss of sleep but further offset freshmen *vs.* junior ability, and in addition produced twice as much work per unit of time with equal accuracy.

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[4] Compare Book, W. F. and Norvell, Lee, "The Will to Learn," *Ped. Sem.*, Vol. 29, 1922, 305-362. Also Book, W. F., *Learning How to Study and Work Effectively*, Ginn and Co., 1926, Chapter XVI.

Other examples of the effect which such an interest regularly produces might be cited. For example, one of the biggest manufacturers in Chicago told his office employees a few years ago that they could have all day Saturday every week for a holiday if they would complete all their work in the remaining five days. They did it easily after that and have enjoyed a five-day week for several years.

These experiments indicate the effect which interest in a task or in one's own advancement really produces and show why we should learn how to become interested in our tasks and work, for interest is a necessary condition for doing any task exceedingly well. We should, therefore, next ask why interest aids a learner or worker in these ways and how such an interest in a particular subject or task like elementary or educational psychology may be developed in an individual or class.

How Interests Are Really Acquired

Our early and strongest interests are, of course, all hereditary, but as Professor James has pointed out, most of the interests of an adult have been acquired during the course of his experience and training. ''An adult man's interests,'' he says, ''are almost every one of them intensely artificial; they have been slowly built up. The objects of our professional interests are most of them in their original nature repulsive; but by their connection with such natively exciting objects as one's personal fortune, one's social responsibilities, and especially by the force of inveterate habit, they grow to be the only things for which in middle life a man profoundly cares.''

This gives us the key to the cultivation of any interests which we may desire to develop in our pupils or in ourselves. We can make our interests practically anything we will if only we know how, for the process takes place according to a few very definite laws. The first and foremost of these laws is the fact that *all new and acquired interests must be built upon the native interests or tendencies to response with which an individual is already endowed.*

This means that if one wants to develop an interest in a particular subject or task he must first of all find something about this new subject or thing that already interests him. For example, the writer once aroused a genuine interest in Latin in a high school boy who was failing in this subject and had learned to thoroughly detest it. In a conference we found that this boy was intensely interested in being a physician and had his future career as a surgeon rather carefully planned. He put in all his spare time loafing at a drug store up town and spent much time visiting a doctor who had taken a personal interest in him. "Harry," the writer said one afternoon when school was dismissed, "I want you to go to Shaptaugh's drug store," naming the place where he regularly spent his time after school, "and copy the names of 50 drugs from the bottles on the shelf and bring me the list tomorrow afternoon. Then I want you to go to Dr. Anderson's office—Harry's favorite physician—and copy from his medical books the names of 50 important diseases and bring that list of names with the other list."

Little more was needed. Practically every name Harry had on the two lists was either a Latin term or a word of direct Latin origin. This fact interested him at once and was a sufficient incentive for him to apply himself, in a gingerly way at first, to the Latin he had learned to detest. As soon as he began to apply himself to the work he not only began to succeed but became genuinely interested because of the action of a second law that governs the acquisition of all interests that we ever acquire.

This law may be stated as follows: *"In order to develop an interest in a subject or task secure much information about it."* This principle is appealed to when an advertiser or salesman gives his prospective customers important information about the article he wishes them to buy. It is the law that has operated in building up in every scientist the keen interest which he has in his specialty. That is to say a scientist's interest consists in large measure of the knowledge that he possesses in a particular field. The writer, for example, was not always interested in psychology, but by the steady accumulation of facts in this field an interest in this subject has been gradually developed that amounts almost to passionate absorption, an interest that has completely overshadowed, even eliminated most of the native interests that formerly dominated his conduct and thought.

If a given piece of work or subject does not in and of itself interest us or our students we should therefore stimulate them to think with intense enthusiasm of the other desirable things to which it may be a means, and to begin at once to get more information about the problems in this field. The former method was used to develop an interest in the 40 printers studied by Dr. Kitson, who made such marked advancement in developing more efficient methods of doing their work. When this experiment began these men had been engaged in the printing trade for an average of 10.3 years. A record of the average amount of work done was taken at the end of the first week after they began to work in the plant, and before the special wage incentive used to interest them in learning to do more and better work began to urge them on to improve their methods of work. The records made during their fourth, eighth, twelfth, sixteenth, and twentieth weeks of service were used to measure any improvement in skill that might have taken place during this time.

One of the rules of this particular industry was that every employee could get more pay for all the work done over a certain amount which was placed, after much experimentation at a point considerably above the skill of the average journeyman printer in the trade. As a result every one of the 40 men whose record was studied boosted his output above this bonus point, the average rate of improvement being 78 per cent. in five months. In other words, the average rate of work of these seasoned printers had been only about three-fourths of what they could easily learn to do if sufficiently interested in making further improvement in their methods of work.

A third law governing the acquisition of interest is the fact that one must arrange matters so that he can and will succeed with all his work. No genuine or lasting pleasure can be attached to a subject, or to the performance of a task, unless the worker is succeeding with his tasks. New things must be stated in terms of the old. The unknown in terms of the known. Unless this principle is followed and the worker is kept succeeding most of the time no genuine interest in the subject can be developed. In other words success is required *to make a student exert himself fully and vigorously towards his tasks* which is the final and most important law that controls the development of what is generally called interest.

The operation of this last law in the development of interest is well illustrated by a story recently told the writer by one of the leading superintendents of schools in the United States. In discussing how interest in a particular school subject might be developed, he related how the best teacher he ever had proceeded to develop it in a class in general science in the high school. This teacher, he said, always began with an object that was thoroughly familiar to every member of his class. One day it was a bean which they put to soak and watched sprout and grow during succeeding days. While this was going on they dissected another bean and determined the structure and function of its several parts: the hard outer shell, the inner covering, and the food portion stored inside these coverings, which the young plant consumed as it grew and developed a way of extracting its food from the soil and the air.

The next week this teacher asked each member of the class to bring a hard boiled egg for inspection and study. All now readily discovered on the basis of what they had done with the bean, the hard protective shell on the outside, the inner lining, and the mass of food on the inside for the embryo chick which they later watched develop in another egg as the experiment proceeded, discovering for themselves a principle which governs an important life process in both the plant and the animal world.

On another occasion the class was given a bit of information about a certain plant that was dissected and studied in class, and two of the pupils were asked to go to a certain swamp some seven miles distant to secure for class use the next day a closely related plant, the skunk cabbage, which they were told was exceedingly rare in that community. Just enough information had been given these boys to arouse their curiosity about the new plant and to enable them to identify it by its peculiar odor. Their native and previously acquired interests were appealed to by having them use the principal's horse and buggy for the seven mile drive after the plant, and by the fact that they were made responsible for securing a new specimen for class use on the following day.

That these and a score of other similar incidents were remembered by this man and by all living members of his class for 30 years when the author of our story recalled them in that city in a commencement address is proof of the keen interest which this great teacher had developed by the simple rules already discussed. (1) First, he had them secure definite information about each subject and object they studied. (2) He always stated the new in terms of what they already knew or had previously done. (3) He made it possible for them to succeed with everything he asked them to do. And (4) he always had his students actively engaged on some project or bit of work, exerting themselves actively towards each problem they took up for study. In fact, so successfully was this done and so great was the interest aroused in each subject taken up in the class that this superintendent's younger brother and mother joined in practically every experiment that was performed in school, an experience which was duplicated in practically every home represented in the class, I have been told.

HOW TO DEVELOP AN INTEREST IN A PARTICULAR SUBJECT OR TASK

Such methods will develop an interest in any subject or task. If, for example, you should desire to develop an interest in educational psychology in a class of college students you must somehow keep them actively engaged in working problems in this field. You must help them apply the knowledge so gained to the study of other subjects and to their own study and work. You must encourage them to choose topics from this field for themes in English Composition, or for conversation and discussion with other members of the class. You must encourage and require them to take an active part in the class discussions, not just lecture to them. The problem consists of keeping them actively engaged in solving problems or in working projects connected with the subject-matter treated in the course and above all you must arrange the work and gauge the assignments so that they can and will succeed with all the work you ask them to do. Presently you shall find that this subject will not only lose all its disagreeable features but fairly bristle with significance and interest, an attitude that serves as a stimulus to urge your students on to still greater activity and success in this direction which, according to the laws already discussed, will create still further enthusiasm and interest, the chief factor in producing success in every field of work.

GENERAL CONCLUSIONS AND RESULTS OF THE CLASS EXPERIMENT

We may now, in closing, state briefly what was done at Indiana University to try to change the unfavorable attitude towards the educational psychology mentioned in the opening paragraph of this report, also state the results obtained in this experiment, and one conclusion that might be drawn on the basis of the facts.

By means already described it was determined what the attitude of the class in question actually was towards this subject when the work they had to do began. We next had them make a careful study of the results that had been obtained in the experiments made by other investigators to ascertain the effect that one's attitude towards his tasks and work actually has upon his ability to do this work. We next worked out what the most favorable attitude towards success, towards one's tasks, and towards one's own advancement really is and determined from the experiments made in the field how much effect these factors and a genuine interest in one's work actually produce when measured in terms of the student's ability to do his work. We also helped them to work out how and why these effects were produced. These students were next given references to read which explained exactly what had to be done to develop such an interest in a particular task or in their work as a whole, and this problem was carefully discussed and illustrated in class.

This changed their attitude completely. They began to apply themselves successfully to the early assignments made in the class which were very carefully worked out by the instructor and enough direction given to enable each student to succeed with every task that we asked him to perform. Care was also taken to point out the practical value and personal significance of the various problems discussed in the class, so far as this could be done. The result was a complete change of attitude, a growing interest in the work of the course and in the problems that were considered from day to day. In fact this method restored the attitude and spirit of these laggard sections to what it had been two years before when the course was an elective and a delight to teach.

It would therefore seem that college students should often be shown by a careful study of the experimental results bearing on the subject the effect of acquiring and maintaining a proper attitude towards their success and towards their own advancement; that they should often be shown that such an interest in their work, a proper attitude towards success, and towards their own progress actually helps them in doing the work they are required to do. If in addition they can be shown exactly how they must proceed to develop such helpful interests and attitudes towards their tasks this group of psychological factors may be fully utilized in bringing about the changes in these students which the teacher desires to make.

READING FIFTEEN[1]

CRITICISMS OF THE LAWS OF EXERCISE AND EFFECT[2]

Editor's Note

In the last Note the inadequacy of association in general was pointed out. However, the principle had its origin in a definite need in the 18th and 19th centuries, and it served a genuine purpose. Psychology had not become a science and was still fraught with mystical and highly speculative deductions. Indeed, there is much of a valid nature that remains from the contributions of association psychology. But the association movement resulted in the formulation of a set of laws, known as the laws of association, that still remain as a part of the psychology of many modern writers. Among them are the laws of exercise (use and disuse), effect and readiness, discussed in the following reading.

Professor E. L. Thorndike, who has been a leader in many fields of psychology in this country, and is now one of its outstanding Educational Psychologists, has been a prominent advocate of associational principles. His recent works show less emphasis upon the laws of use and disuse, if not altogether a repudiation of them. He has always emphasized certain concepts that fit well into organismic theory, as for example, the response of organisms to total situations, and the importance of goals, but their organismic significance has been clouded by a constant effort to account for the complex forms of behavior in terms of the simple.

The inclusion, here, of Professor Cason's criticisms of Professor Thorndike's views must be interpreted not as a comment on Professor Thorndike but upon a type of psychological theory whose popularity is even now much greater than Professor Thorndike himself would probably think justified. Physiological knowledge has advanced considerably since the quotations given by Professor Cason were written, and many experiments, several carried out under Professor Thorndike's own direction, point to the questionable nature of the laws of use and disuse.

In order to understand the principles which Professor Thorndike expressed in terms of conduction in nerve cells, a detailed knowledge of the nervous system is not necessary. It need only be understood that neurons are anatomically arranged in extremely complicated chains and net-works, and that the junctures between cells, called synapses, are places where the discharging end of one cell, the axon-terminal, comes in very close proximity to the receiving end, the dendrite, of another cell. It was long supposed that repetition of conduction lowered the resistance offered by the synapse to the passage of nerve currents through it; thus repetition of an act made the performance easier. On the contrary, in the light of recent evidence to be presented in later readings, it seems more likely that any synapse, once formed in the course of maturation of the nervous system, is ready to function perfectly the first time, and that use does not affect it. Moreover there is evidence that progress in the learning process is a consequence of maturation and the innervating character of outside forces acting upon the organism, not a consequence of repetition of response, or use. If this is true, repetition of response, when under control, bears the same relation to learning that time bears to the general growth of the organism. Repetition of response toward a more and more definite goal is always present; there is no learning without it any more than there is growth without time. But time will not explain growth.

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[1] Parallels *The Science of Psychology*, Chapter IX.

[2] By Hulsey Cason, University of Rochester. From the *Psychological Review*, Vol. 31, No. 5, 1924, 397-417. By permission.

1. Introduction

The laws of exercise and effect have become a part of the psychology of many modern writers, and they have attained a prominence which we think is not entirely justified. It is our purpose in the present paper to criticize both of these laws, and attempt to show that they do not fit the facts of learning. The apparent confusion in regard to the laws of exercise and effect seems to arise partly from the assumption of such non-physical entities as mind and consciousness, and partly from a somewhat premature attempt to describe the physical "correlates" of satisfyingness and annoyingness. The history of association psychology shows the futility of attempting to build a science on a mentalistic assumption. In contrast with the association psychology of former years, we assume in the present paper that an association is between a stimulus and a response, and not between two ideas or two external objects.

2. The Law of Exercise

In the present section, in which we attempt to criticize the law of exercise, we shall (1) state the law of use, which is a part of the law of exercise, (2) attempt to show that some other factor is always present in learning in addition to the mere exercise of a single stimulus and a single response, as postulated in the law of use, (3) criticize the common analogy of the muscle, which is also said to improve with "exercise," (4) attempt to show the improbability of the physical mechanism postulated for the law of use, and (5) discuss and criticize the law of disuse.

The law of exercise includes the laws of use and disuse. We shall now attempt to show that the *law of use,* which states simply that the exercise of a function improves it is not an accurate statement of fact.

Thorndike's Statement of the Law of Use.—In the *Original Nature of Man,* Professor Thorndike states the law of use as follows: "To the situation, 'a modifiable connection being made by him between a situation *S* and a response *R,*' man responds originally, other things being equal, by an increase in the strength of that connection. By the strength of a connection is meant the probability that it will be made when the situation recurs. Greater probability that a connection will be made means a greater probability for the same time, or an equal probability but for a longer time. Thus, strengthening the connection between 'being asked how many six and seven are' and 'saying "thirteen,"' may mean that the probability of that response during the next six days is eight out of ten instead of seven out of ten, or that the probability is seven out of ten for sixty days instead of for forty."[8] The statement of the law of use in Professor Thorndike's *Psychology of Learning* is substantially the same.

Woodworth's Statement of the Law of Exercise.—Professor Woodworth, in discussing the law of exercise, writes as follows: "Of one law of learning, we are perfectly sure. There is no doubt that the exercise of a reaction strengthens it, makes it more precise and more smooth-running, and gives it an advantage over alternative reactions which have not been exercised. . . . This law is sometimes called the 'law of habit,' but might better be called the 'law of improvement of a reaction through exercise,' or, more briefly, the 'law of exercise.' The law of exercise is very broad in its scope, holding good of life generally and not alone of mental life. Exercise of a muscle develops the muscle, exercise of a gland develops the gland; and, in the same way, exercise of a mental reaction strengthens the machinery used in making that reaction. Let us restate the law in terms of stimulus and response. *When a given stimulus arouses a certain response, the linkage between that stimulus and that response is improved by the exercise so obtained,* and thereafter the stimulus arouses the response more surely, more promptly, more strongly than before."[4] Professor Woodworth, in a further discussion, treats the neural basis of the law of exercise: "Exercise, then, has the same general effect on neurons that it has on muscles; it causes them to grow and it probably also improves their internal condition so that they act more readily and more strongly. The growth, in the cortex, of dendrites and of the end-brushes of axons that interlace with the dendrites, must improve the synapses between one neuron and another, and thus make better conduction paths between one part of the cortex and another, and also between the cortex and the lower sensory and motor centers. The law of exercise has thus a very definite meaning when translated into neural terms. It means that the synapses between stimulus and response are so improved, when traversed by nerve currents in the making of a reaction, that nerve currents can get across them more easily the next time. The more a synapse is used, the better synapse it becomes, and the better linkage it provides between some stimulus and some response. The cortex is the place where linkages are made in the process of learning, and it is there also that forgetting, or atrophy, takes place through disuse. Exercise makes a synapse closer, disuse lets it relapse into a loose and poorly conducting state."[5]

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[8] Thorndike, E. L., *The Original Nature of Man,* 171-172.

Limitations of the Law of Use.—The law of use has never purported to explain how associations are originally formed. We may now attempt to point out other limitations, and some inconsistencies as well. Some other factors seem to be necessary besides the mere exercise of a specific reaction-arc to improve the strength of the association. To illustrate, we may quote Professor Woodworth's account of the incident that occurred in a Swiss psychological laboratory: "There is a famous incident that occurred in a Swiss psychological laboratory, when a foreign student was supposed to be memorizing a list of nonsense syllables. After the list had been passed before him many times without his giving the expected signal that he was ready to recite, the experimenter remarked that he seemed to be having trouble in memorizing the syllables. 'Oh! I didn't understand that I was to learn them,' he said, and it was found that, in fact, he had made almost no progress towards learning the list. He had been observing the separate syllables, with no effort to connect them into a series.''[6] Such facts show that the law of use is not a safe law to depend upon in practical affairs. This foreign student was repeating the nonsense syllables in the proper order, but although there was much exercise, there was little learning. Obviously, then, his practise was not of the proper kind, and the claim might be made that he did not exercise the proper neurons. Immature students sometimes follow the lines of a printed page without knowing very much about the material covered. Studies on the ''value of recitation'' also illustrate the fact that something else besides mere repetition is necessary. It is claimed that the student must intend to learn the material, must have the will to learn, and be interested in the task. These considerations show at least that the mere outward performance of an act is not necessarily an accurate indication of the amount of strengthening of the connection between the stimulus and the response.

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[4] Woodworth, R. S., *Psychology, a Study of Mental Life*, 389-390.

[5] *Op. cit.*, 414-415.

The Analogy of the Muscle.—It is commonly said that the exercise of a mental function improves the mental function, just as the exercise of a muscle improves the muscle; but this does not seem to us to be an accurate statement. We wish just now mainly to stress the fact that *outside factors* influence the muscle. Some products are brought in and other products are taken out, so that the muscle is not left in exactly the same condition as it was before. In fact, it is not exactly the same muscle. The popular statement ignores these important outside influences. The intrinsic and extrinsic muscles of the eye do not improve with exercise. The outside factors in some cases may cause muscles to become weaker rather than stronger, as when the athlete ''goes stale.'' Fatigue may mask the desirable effects, while strain and overexertion may completely destroy them. Exercise does not ordinarily improve many organs and functions of the body, *e.g.*, swallowing, coughing, sneezing, breathing, heart-beat, winking, child-bearing, etc. Sense organs also do not continue to improve with practise and age.

It has been claimed that practise on a typewriter improves the typewriting; but we know that the function at the end is quite different from the function at the beginning of a distributed practise of, for example, 200 hours. It is true of course that the subject has a typewriter before him during all of the practise, he is in a sitting position, he may be using his hands, etc.; but different methods are used and different attitudes are taken; the specific reaction-arcs are different at the beginning and at the end. Some other processes must be considered in order to explain satisfactorily the improvement in typewriting besides the mere exercise of the original specific reaction-arcs.

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[6] *Op. cit.*, 346.

Improbability of the Physical Mechanism Postulated for the Law of Use.—The law of use does not hold in biology generally, and it may be readily seen that the physical sciences also afford no data which illustrate it. For example, after electricity has passed through a copper wire, the wire is generally in no better shape to conduct the second time—in fact, it may be in slightly poorer shape. The force of gravity produces no greater effect on an object the second time. If the strength of the electrical attraction between two bodies is affected, then one or both of the two bodies (or the relation between them) must have been changed.

The experimentally demonstrated facts of nerve physiology also do not indicate that an impulse will pass across a synapse or along a nerve trunk any better the second time. Use seems to make very little difference, and certainly not enough to account for the speed of learning and the improvement in various functions, which are commonly observed. The work of C. U. A. Kappers and others on neurobiotaxis shows that the mere exercise of a specific reflex arc does not improve the arc. Stimulation is necessary for the *growth* of nerve fibers, to be sure, but the stimulation comes from the *outside* rather than from the *inside* of the specific reaction-arcs involved.

Exceptions to the Law of Use.—The experiments on the conditioned reaction and negative adaptation afford numerous exceptions to this law.[7] In Pawlow's well-known experiment, it was observed that the dog's saliva would not continue to flow with repeated repetitions of the bell stimulus. Several others have made a similar observation. Some very familiar examples of the same thing are also available from everyday experience. Suppose that an association is formed between a snake and a fear reaction, in a child. Now if this child in later life catches snakes and plays with them and operates on them, etc., this fear response will in all probability degenerate; but, according to the law of use, the fear response should become more pronounced.

[7] It is assumed in these illustrations that the stimulus and the reaction are already associated. What we wish to criticize is the assumption that use will improve these connections.

A great many dislikes, aversions, fears, etc., are broken down in this manner. As in former illustrations, outside factors influence the specific reaction-arcs involved in the act.

The examples of negative adaptation are similar to those offered above. We are annoyed at first by the noise of the city, but may soon become accustomed to it. We hear the clock one moment, but are not aware of it the next. Some other factors are at work besides the mere exercise of the specific connections which were involved just after the connection was originally formed.[8]

The Law of Disuse.—This law is stated by Professor Thorndike as follows: "To the situation, 'a modifiable connection not being made by him between a situation *S* and a response *R*, during a length of time *T*,' man responds originally, other things being equal, by a decrease in the strength of that connection."[9] However, it may be pointed out that there is sometimes a strengthening and sometimes a weakening of a modifiable bond, during "disuse." It may be either the one or the other; and the "other things" are never equal. The bond is *not necessarily* weakened during disuse; and, if it should become weaker during disuse, it is not necessarily weakened because of the disuse, but because of outside factors which influenced the specific reaction-arcs. Fatigue products may be removed, and the bond may be *strengthened* during disuse. Time brings about changes in all organic compounds but the change is not necessarily for the *worse*. Furthermore, shock and the death of nerve cells, etc., may also weaken a connection, so that it is not always weakened *because of* the disuse.

Elsewhere, Professor Thorndike states the "law of disuse" as follows: "When a modifiable connection is *not* made between a situation and a response during a length of time, that connection's strength is decreased. The explanations and qualifications stated in connection with the Law of Use apply here also."[10] Apparently *all* of the explanations and qualifications which Professor Thorndike stated in connection with the law of use cannot apply to his statement of the law of disuse as well. We presume, however, that by "a modifiable connection" Professor Thorndike has reference to a change in the strength of the bond. Stated in this way, the law cannot be correct, since during disuse the bond is *sometimes strengthened* by outside factors. Your eyes may seem weak tonight, and stronger tomorrow. You may be too tired to work just now, but rest will get you back in shape again. You may not be able to recall a name now, but may recall it all right in five minutes. Outside factors have an influence, and the strength of the bond is not necessarily decreased when it is not "exercised."

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[8] Of course there are many *degrees* of strength of connections between stimuli and responses; and it is desirable to know something about the amount and extent of the association, as well as whether there is any connection at all. There may be *some* connection, to be sure, between stimulus *A* and response *B* which was learned some twenty years ago, but it would be somewhat arbitrary to say that there *is* or that there *is not* a connection between *A* and *B* at the present time.

[9] Thorndike, E. L., *The Original Nature of Man*, 172.

[10] Thorndike, E. L., *The Psychology of Learning*, 4.

If by the statement, "when a modifiable connection is *not* made between a situation and a response during a length of time, that connection's strength is decreased," Professor Thorndike means that no bond is present at all, then of course the strength of that bond could not have been *decreased,* since it was zero at the start. If Professor Thorndike has reference to an antagonistic response, then it seems to us that the strength of the response in question would not be decreased; but, rather, the strength of the antagonistic response would be increased.

Professor Thorndike would doubtless apply the term "situation" to the total condition of the organism, both internal and external, *all* factors being considered. Now if there is no change whatever in the relation between the total situation and the response—which of course must be most intimately related to the whole situation, and, in fact, be a part of it—we do not believe there could possibly be a "decrease in the strength of the connection." We agree with Professor Thorndike and others in the theoretical statement that the same situation always calls out the same response, and we therefore do not understand how it can be possible for the same situation to call out a different response in this particular case.

It is impossible to know just what is meant by some of the available statements of the law of *disuse,* and this is the reason that we have attempted to show: (1) that the law of disuse never has a chance to operate, (2) if it did have a chance to operate it would not operate as claimed, (3) if it did function it would function for a different reason than the one stated, and (4) if it functioned at all it might work in just the opposite direction from the one described.

It is apparently impossible to visualize the physical mechanism of the law of exercise. The law of use and the law of disuse are both contradictory and misleading. We must take account of other causal factors besides those present on the *inside* of the specific reaction-arcs.

3. THE LAW OF EFFECT

In this section, we shall attempt to criticize the law of effect from various points of view. We shall (1) state the law of effect as formulated by Professor Thorndike, its leading exponent, (2) state and criticize one of the best examples of the practical operation of the law of effect, as given by Professor Gates, (3) criticize the law of effect from the point of view of the psychology of feeling, (4) state and criticize the law of readiness, which is part of the law of effect, and (5) attempt to show that some doubtful assumptions have been forced in connection with sensory pains. The law of effect has so many sides, and is so involved and technical, that it is difficult to give a well organized criticism. Throughout this section, however, we are always attempting to give evidence against the law of effect from one point of view or another.

The law of effect has been proposed as an explanation of the formation of associations. Pleasure is said to ''stamp in'' the successful reaction, and displeasure is supposed to ''stamp out'' the unsuccessful response. It is claimed that the satisfyingness and annoyingness of the act operate as selective agencies, eliminating the useless movements. Pleasure and pain are thus looked upon as substantive faculties exerting an influence on behavior. We believe that if these terms were defined in more scientific language much of our difficulty concerning them would disappear, and we would find that, after all, what we meant to describe were certain kinds of reactions of the human organism. It is very difficult to understand how two general *faculties* could operate so persistently in one body, and work antagonistically to each other. Few writers have attempted to describe the way these faculties are supposed to operate. The supposition that either the one or the other is always at work *when associations are being formed* has also not been justified. Reaction tendencies are sometimes modified with very little, if any, pleasure or pain.

We may quote the following statement from Professor Thorndike, which shows the influence of psychological hedonism: ''Reason finds the aim of human life the improvement and satisfaction of wants. By reducing those to which the nature of things and men denies satisfaction, or by increasing those which can be fulfilled without injuring the fate of others, man makes his wants better. By changing the environment into a nature more hospitable to the activities he craves, he satisfies them. The sciences and arts arose by the impetus of wants, and continue in their service. They are the ultimate source of all values. The original basis of the wants which so truly do and should rule the world is the original satisfyingness of some states of affairs and annoyingness of others. Out of such original satisfiers and annoyers grow all desires and aversions; and in such are found the first guides of learning.''[11] We would prefer to say that the sciences and arts arose partly by the impetus of *activities* of the human organism, rather than by the impetus of *wants*. The wants are also not the ultimate source of *all* values, for many things are valuable which we do not want at all. Furthermore, the world is not ruled by ''wants.'' Professor Thorndike holds, we think, erroneously, that the ''original basis'' of wants is the ''original satisfyingness of some states of affairs and annoyingness of others.''

Satisfyingness and Annoyingness are defined by Professor Thorndike as follows: ''By a satisfying state of affairs is meant roughly one which the animal does nothing to avoid, often doing such things as attain and preserve it. By an annoying state of affairs is meant roughly one which the animal avoids or changes.''[12] A good many reactions of the organism would come under this category, but some of them would not be in the least ''satisfying,'' as the term is used in popular speech. We are not at all aware of many of our physical activities. If Professor Thorndike means that the satisfying reaction is simply the one which the animal makes, then the application of the law of effect would be a circular argument: for the animal would then be said to make the reaction because it is satisfying, and a satisfying reaction in turn would be said to be the one which the animal makes.

Relation to the Psychology of Feeling.—The affections and the feelings have given some encouragement to those who believe in the algedonic theory. All feelings are said to be either pleasant or unpleasant; but there are numerous contradictions in the introspective work in this field; and we have at least some reason for believing that if feelings are present at all: (1) they may be "mixed," (2) they may at times be more or less negative even in regard to pleasantness and unpleasantness, and (3) the feelings may be made up simply of sensations. Considerable difficulty has been experienced in identifying the physical "correlates" of these processes, and it is generally understood that the whole subject of feeling is in a bad condition. Several people testify that when they are learning some things they feel neither pleased nor displeased, but just negative, and certainly all people must lapse into this condition at one time or another while the learning process is active. We may not have many feelings when we are reading, or they may even be unpleasant, but associations are being formed just the same. Feeling and learning are certainly not very closely correlated.

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[11] Thorndike, E. L., *The Original Nature of Man*, 123.
[12] *Op. cit.*, 123.

Illustration of the Operation of the Law of Effect.—Professor Gates has given a good illustration of the way the law of effect is supposed to work. Briefly stated, suppose three cats have been taught to come to you when you call them. Now call them individually. Feed No. 1, act indifferently toward No. 2, and throw a bucket of water on No. 3. "Other things being equal," Professor Gates says that the *law of exercise* will not apply, since the three cats will thereafter behave differently when you call them. No. 1 will probably come more surely and promptly. No. 2 will "continue to respond for a while, but less promptly and frequently, finally failing altogether." No. 3 will "probably very promptly show a failure to respond."[13] Discussing this behavior, Professor Gates writes as follows: "Apparently there is something about the *effect* which accompanies or follows an act that either reinforces or decreases the results of use or causes an animal to repeat the act in one case and avoid it in the other, or both. Pain, confinement, the holding up of a tendency under way, futile or wasteful work all tend to stamp out the reactions which they accompany or follow. On the other hand, reactions which bring release from confinement or pain, which bring food, kindly treatment, or attention, are repeated and stamped in more rapidly than when they are exercised with an indifferent result. It is the reaction which brings, broadly speaking, a satisfying state of affairs, absolute or relative, that is repeated and stamped in quickly during use. The more satisfying the resulting state the more surely the reaction will be repeated and the more quickly stamped in. On the other hand, those reactions which are avoided or are stamped out even during the process of exercise, are the ones which are accompanied or followed by annoying states of affairs or annoying effects.''[14] We admit that there is *something* which caused the changed type of behavior, but we do not understand how this could be the ''satisfaction,'' used either in the popular sense (which is apparently the case in the above quotation), or used in the other sense, as existing in the mind. Professor Gates follows Professor Thorndike in the definition of satisfaction, stated above. The criticism which we suggested against Professor Thorndike's views applies also to Professor Gates': if satisfyingness is conceived as a disembodied entity, then the law of effect as it has been stated cannot be accepted; whereas if a satisfying state of affairs means just what the animal does, then this statement of the law of effect runs in a circle. We have yet to explain why the three cats changed their ways.

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[13] See Gates, A. I., *Psychology for Students of Education*, 227-228.

If the everyday concept of satisfyingness, satisfaction, pleasant, pleasure, agreeable, etc., is intended, in the discussions of the ''stamping in'' and ''stamping out'' effects, then we find equal difficulty in accepting the law of effect. We know practically nothing about the ''subjective'' feelings of animals below man; and human learning shows many exceptions to this proposition, since so much learning is effected without any particular feeling tone being present. Certain writers claim that the most efficient learning is the ''unemotional learning.'' Excitement and worry are not conducive to the best performances, they say. Now whether the ''pleasantness'' is present or not, certainly the formation of associations is not dependent solely upon the presence of this pleasantness or unpleasantness, but upon *certain physical activities of the organism.* If pleasantness is defined in terms of physical reactions of a physical organism, then it may be said that these reactions are not the *only* reactions which caused a change in behavior of the three cats referred to above. It is an unjustified assumption to suppose that *these* ''pleasantness'' and ''annoyingness'' reactions are *the* reactions which change the behavior, and that no other activities in the organism are *in the least* responsible for the modified reaction tendencies. If pleasantness is defined in terms of a dualistic psychology, as a non-physical entity, then we still have some difficulty in accepting the various statements of the law of effect.

Native Likes and Dislikes.—The reflex activities of the newly-born child tell us very little about the feelings which he experiences, if indeed he has any feelings at all. In a vague sort of way the feelings of an adult can be judged from his facial expression, etc., but in the case of the child just born, and in the case of animals below man, practically nothing is known about affection and feeling. The one-year-old child is very probably not particularly pleased when he waves his hands, nor particularly displeased when he cries. These are simply reflex responses, and it would be unscientific to judge his feelings by his behavior, especially since there is such a strong tendency to read your feelings into his acts. The "conscious aspect" of various reactions seems to be built up very gradually, and it would be erroneous to suppose that the conscious reactions themselves are the sole determiners of all of the acts which are learned in the early years of infancy. Although the infant makes certain avoiding or defense movements, some of the stimuli which originally call out these defense movements may later call out responses which are pleasant. Likes and dislikes change in such a peculiar fashion from day to day.

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[14] *Op. cit.*, 228.

In these days of skepticism concerning instincts, it seems plausible to the writer that conscious feeling reactions may themselves be learned. The likes, dislikes, pleasures, and aversions of people differ so enormously that no one would claim that they are *entirely inherited.* But, we may ask, what becomes of the *selective activity* of satisfyingness and annoyingness while the satisfyingness and annoyingness are themselves being learned? If satisfyingness and annoyingness are learned reactions, or just partly learned, then obviously the acquired satisfyingness and annoyingness could not possibly have been acting as a selective agency in *acquiring itself.* We have to conclude that satisfyingness and annoyingness are *completely inherited,* or that some other factors besides satisfyingness and annoyingness have been responsible for some or all of the learning which has taken place. All of the facts indicate that the latter view is the correct one.

A Concept of Readiness.—In considering the question of the elimination of useless movements in animals below man, some psychologists have pointed out that the rat in the maze cannot distinguish between the "successful" and the "unsuccessful" turns at the time he makes them, and of course a human subject in the same situation would have a similar difficulty. One turn looks like another to the rat at the time he makes it, and it is difficult to understand how the *successful* turn could have been "stamped in" and the *unsuccessful* turn "stamped out," at the time they were made.

We return to a statement of this question by Professor Thorndike, who writes as follows: "Successful operation can in fact be satisfactorily defined, and what will originally satisfy and annoy can be safely predicted, only as a characteristic of the internal behavior of the neurons. By original nature a certain situation starts a behavior-series: this involves not only actual conduction along certain neurons and across certain synapses, but also *the readiness of others to conduct.* The sight of the prey makes the animal run after it, and also puts the conductions and connections involved in jumping upon it when near into a state of excitability or readiness to be made. . . . The activities of the neurons which cause behavior are by original nature often arranged in long series involving all degrees of *preparedness* for connection-making on the part of some as well as *actual* connection-making on the part of others. When a child sees an attractive object at a distance, his neurons may be said to prophetically prepare for the whole series of fixating it with the eyes, running toward it, seeing it within reach, grasping, feeling it in his hand, and curiously manipulating it."[15] We cannot agree that "what will originally satisfy and annoy can be safely predicted, only as a characteristic of the *internal* behavior of the neurons," *i.e.,* granted that anything will *originally satisfy.* Even if we should suppose that some things are originally satisfying or annoying, we would certainly not limit the total process to the *internal* behavior of neurons. It is a problem of the future even to show that conscious reactions are entirely dependent upon activity *in neurons* or in nervous tissue. The stimuli, sense organs, nerve-trunks, the nature of the synapses, muscles, glands, etc., must all be considered. In the statement quoted above, Professor Thorndike fails entirely to take account of the fact that *afferent impulses frequently come from muscles, thereby influencing behavior.* According to Professor Thorndike's view, we wait for an *instinct* to start the behavior-series; and then this instinctive activity changes the "readiness to conduct" of other reaction-arcs. While it is true that activity in reaction-arc *A* may cause impulses to *pass through* reaction-arc *B*—chain reflexes, circular reflexes, etc.—and while activity in *A* may even alter some of the synapses in *B*, there are a great many exceptions, and sometimes it does not work the "right" way: *B may be* inhibited rather than innervated. Professor Thorndike's statement suggests that the neurons always operate in some unusual way to bring about a *successful* result. Neurons, as far as we know, behave in strict accordance with physical and physiological laws; and a vitalistic assumption is unwarranted and unnecessary even in the present state of our knowledge of nerve physiology.

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[15] Thorndike, E. L., *The Original Nature of Man,* 125-126.

We should apparently not let the matter rest here, for Professor Thorndike goes on in the next few sentences to say: "The fact is that it is the neurons, not the body as a whole, whose life processes are primarily concerned in the "successful" operation of a behavior-series. By "normal" or "successful" operation we mean the externally observable signs of the action of neurons that are ready to act. And by the failure, or thwarting, of an original tendency we mean the observable signs of failure to conduct and connect in neurons which are ready to act."[16] We might contract the first two sentences to read as follows: "Not the body as a whole, but the life processes of the neurons, are primarily concerned in the externally observable signs of the action of neurons that are ready to act." Or, still more briefly: "The neurons are primarily concerned in behavior due to the action of neurons." More briefly still: "Neurons are concerned in the effects of neurons!" In the meantime, the question of the "successful" response has been sidetracked.

The Law of Readiness.—This is stated by Professor Thorndike as follows: "The essential satisfyingness[17] in these cases is then the conduction along neurons and across synapses that are ready for conduction and the essential annoyingness[18] in these cases is the absence of such conduction."[19] Professor Thorndike states further that this law "holds good not only in the case of such definite behavior-series as feeding, hunting, fighting or sex-indulgence, but throughout behavior.''

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[16] *Op. cit.*, 126-127.

[17] According to Professor Thorndike, "Samples of original satisfiers or instinctive likes are:—*to be with other human beings rather than alone, to be with familiar human beings rather than with strange ones, to move when refreshed, to rest when tired, to be 'not altogether unenclosed' when resting at night*." (*Op. cit.*, 123.)

[18] For Professor Thorndike, "Samples of original annoyers or instinctive aversions are: *bitter substances in the mouth, being checked in locomotion by an obstacle, being hungry, being looked at with scorn by other men, the sight and smell of 'excrementitious and putrid things, blood, pus, entrails.'*" (*Op. cit.*, 123.)

[19] *Op. cit.*, 127.

The Laws of Readiness and Unreadiness.—Let us quote Professor Thorndike again: ''I believe that the original tendencies of man to be satisfied and to be annoyed—to welcome and reject—are described by these three laws of readiness and unreadiness:—(1) that *when a conduction unit is ready to conduct, conduction by it is satisfying, nothing being done to alter its action,* (2) that *for a conduction unit ready to conduct not to conduct is annoying, and provokes whatever responses nature provides in connection with that particular annoying lack;* (3) that *when a conduction unit unready for conduction is forced to conduct, conduction by it is annoying.''*[20] We may consider these three laws separately:

(1) So little is known about the physiological basis of the feelings of pleasantness and unpleasantness that it is somewhat premature to conclude the matter so abruptly and to say that when a conduction unit is ''ready'' to conduct for *it* to conduct is satisfying to the *organism as a whole.*

(2) It is highly improbable that the failure to conduct of a ''conduction unit''[21] is annoying. If by readiness is meant merely a physical state, then surely failure to exercise this *particular connection* would not cause displeasure to the *whole organism,* for consider all the neurons in the cerebrum that are lying ''idle'' and ''waiting'' for the proper stimuli to call up memories and facts which have been learned in the past. The only other alternative is to attribute *human* qualities to the conduction units, and to think of them as ''ready'' or ''not ready,'' just as when we say ''I am ready to go'' or ''I am not ready to go.'' We hesitate to believe that Professor Thorndike would advocate such anthropomorphism as this.

Now suppose that one conduction unit in the body is ready to conduct and conducts; and suppose further that (while this is happening) another conduction unit in the same body is not ready to conduct, but conducts just the same. That is to say, suppose that satisfyingness and annoyingness are produced at the same time and in equal quantities, then, we may ask, does one smother the other, or is the effect somewhat like the component action of several forces? Satisfyingness is said to be

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[20] *Op. cit.*, 128.

[21] "Neuron, neurons, synapse, synapses, part of a neuron, part of a synapse, parts of neurons or parts of synapses." (*Op. cit.*, 127.)

"what the animal tries to attain" (positive tendency), and annoyingness is said to be "what the animal tries to avoid" (negative tendency). Although the animal may be torn by these two tendencies, he could not be in two places at once, and we have to conclude that no "effect" would be produced in this hypothetical case, although one conduction unit was ready to conduct and conducted and the other conduction unit was not ready to conduct but conducted just the same. "Readiness to conduct" would apparently not *always* produce an "effect."

(3) To force a conduction unit to conduct when it is "not ready" to conduct! Either the criticism in regard to anthropomorphism applies here or Professor Thorndike must conceive readiness in purely physical terms. If the latter is the case, then we do not understand how a conduction unit could possibly be *forced to conduct* when it is *not ready* to conduct. If the conduction unit is in proper shape to conduct, assuming that it gets the stimulus, it *will* conduct; and if it is not in proper shape to conduct, assuming again that it gets the stimulus, it *cannot* conduct. We may once more use the analogy of the copper wire, which *will* conduct an electric current if certain physical requirements are met; but if these physical requirements are not met, for example, if the wire is broken, it *will not* conduct. There is no such thing as forcing this wire to conduct "against its will." It is exactly the same with the reaction-arcs of the human machine.

Sensory Pains are considered by Professor Thorndike as simply one class of annoyers.[22] "It is necessary to suppose that the conduction units whose action causes sensations of pain are almost always unready to conduct."[23] But some pains are welcomed as the lesser of two evils, for example, "A man knowing that pain in his eyes would mean that he was cured of threatening blindness might well cherish that pain when it came."[24] In such a case as this, however, Professor Thorndike says that "We should expect that the conduction units concerned *would* be made ready." In ordinary conditions, therefore, if a man experiences a pain, "the conduction units whose action causes sensations of pain are almost always unready to conduct"; but if the man welcomes the pain, as in the above illustration, Professor Thorndike assumes that these same conduction units, whose action causes sensations of pain, are now *ready* to conduct. These neurons, according to Professor Thorndike, carry *pain impulses* regardless of whether the man welcomes or avoids the pain, but in the one case they are ready to conduct and in the other case they are unready to conduct! According to Professor Thorndike, the *readiness rather than the pain determines the man's behavior,* in such cases as these.

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[22] *Op. cit.*, 124, 128-129.

[23] *Op. cit.*, 129.

[24] *Op. cit.*, 129.

We are unable to visualize the (physical) mechanism which could mediate or cause this peculiar kind of behavior. Why should reflex-arcs *continue* to conduct pain impulses if they are "unready to conduct"? As a matter of fact, some of these neurons seem to be fairly efficient in conducting pain impulses, as in the case of those neurons which conduct pain impulses from the teeth and ears. It is difficult to conceive of these neurons as *unready* in any sense of the word. Furthermore, the pain receptors, probably the intraepithelial nerve-endings, are *very sensitive organs.* The well-known phenomenon of sensory adaptation is *practically absent* in the pain sense. Sensations proceeding from the pain organs are *not necessarily* painful in the sense that they are disturbing or unpleasant. The blending of sensations of pain with sensations of pressure and temperature affords another interesting problem.

Professor Thorndike admits that there is no conclusive evidence for his assumption, but says that *some* evidence is furnished by the fact that reaction-time to pain is slow. It seems to the writer that *all* of the available evidence is on the side which is against this theory. The *speed* of a reaction is not a true indication of the "readiness to conduct," which is said to determine the satisfaction, which in turn is said to determine the learned behavior. According to this view, all learned behavior would be a function of the speed of conduction in reflex-arcs. Reaction-time is dependent upon a variety of factors: as, for example, the nature of the receptors, the number and length of the neurons involved, disturbing elements (such as would be present in sensory pains), etc. Furthermore, some slow activities in the body are pleasant and some fast activities are unpleasant. Various activities go on in the body with various speeds, and we are not at all aware of many of them. It is very difficult to understand how speed could be so closely correlated with "readiness" in some neurons but not in others.

The law of effect is in no way a plausible explanation of association and learning. The principal fallacy in Professor

Thorndike's statement of this law is the premature attempt to describe the physical basis of the "awareness of pain" and the "awareness of pleasure." Psychology is not yet ready to give a plausible explanation of such experiences.

4. Conclusion

We may now attempt to give a brief summary of the preceding paragraphs:

1. The law of use, which states that the exercise of a function improves it, is faulty in the following respects: (*a*) it ignores the outside factors which influence the specific reaction-arcs, (*b*) it does not always operate as claimed, and (*c*) it is impossible to conceive of the physical mechanism involved.

2. The law of disuse, or the general principle of atrophy through disuse, has been rejected for practically the same reasons as the law of use. In discussing learning problems, it is necessary to take account of other causal factors besides those present on the inside of the specific reaction-arcs.

3. The law of effect, which purports to explain the formation of associations, is based upon psychological hedonism. This doctrine can no longer be justified from the side of philosophy.

4. Satisfyingness and annoyingness are conceived as substantive faculties exerting an influence on behavior. They are said to act as selective agencies, stamping in the "desirable" reactions and stamping out the "undesirable" reactions. These propositions cannot be accepted.

5. Satisfyingness and annoyingness are said to be entirely inherited, and proper attention is not paid to those feeling reactions which are learned.

6. The laws of readiness and unreadiness are anthropomorphic. It is highly improbable that the readiness or unreadiness of certain specific reaction-arcs to conduct can influence the satisfaction of the whole organism.

The laws of exercise and effect have been used as fundamental concepts especially by those who are interested in the applications of psychology. Some of these writers claim that all influences of the environment on the individual operate through these laws; and it is obviously of importance to technical psychology as well as to educational theory and practise to be clear on these matters. We feel that the criticisms which we have offered above are concerned with much more than incidental and arbitrary points of terminology.

The law of effect is based upon the insecure pleasure-pain philosophy, and postulates entities which have no existence in fact. It is mentalistic and anthropomorphic, and forces those who believe in it to accept some very doubtful propositions in connection with sensory pains and the psychology of feeling.

It may seem to the reader that we have been somewhat impolite in thus criticizing two important laws of learning, without suggesting anything in their place. Probably no theory of learning can be entirely justified at the present time; and we could not even make an attempt at such a justification in the space which is here available. We are convinced, however, that the theory which will eventually be evolved will be one which breaks completely with mentalism and the pleasure-pain philosophy. For a theory of learning to be scientific, it must take proper account of what is known about nerve physiology, and the physical and chemical relations of the organism.

GLOSSARY

Afferent, conducting inward from the sense organs to the brain.
Algedonic, pertaining to the feelings of pain and pleasure.
Anthropomorphism, interpreting events in terms of human experience; giving an object human characteristics.
Efferent, conducting outward from the brain to the muscles.
Hedonic, pertaining to the feeling of pleasure.
Innervated, incited to action; stimulated.
Mentalistic, assuming a vital principle of a unique order as the basis of mental life.
Neurobiotaxis, the movements and growth of dendrite and axon terminals; migrations of nerve cells especially during embryonic growth.

NOTE ON INSIGHT IN HELEN KELLER

By Thomas D. Cutsforth

[The following note by Dr. Thomas D. Cutsforth is here inserted to demonstrate the rôle of insight in the learning process and to illustrate the *law of individuation.* Ed.]

Miss Sullivan undertook the education of the seven-year-old, deaf-blind daughter of Captain Keller in the spring of 1887. In a letter, dated April fifth of that year,[1] Miss Sullivan gives a clear account of the situation in which Helen became aware of the existence of language and learned that everything has a name.

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[1] Helen Keller, *The Story of My Life*, edited by John Albert Macy, Doubleday, Page and Company, 1920, 315-316.

Prior to the arrival of her teacher, Helen communicated with her family by the use of signs and gestures. Miss Sullivan, employing the same method of instruction by which Dr. Howe taught Laura Bridgman, in about a month, had substituted letters of the manual alphabet for the signs and gestures for several situations. On the third of April, Helen had a vocabulary of eighteen nouns and three verbs. That is to say, manual alphabet-words represented twenty-one different situations to Helen. For example, the letters m-u-g, spelled out on the hand, did not mean the specific object, mug, *but the total situation of milk being poured into the mug, milk or water in the mug, and that Helen wanted a drink.* The specific meaning of *mug* had not yet been differentiated out of the total situation. Helen's teacher had begun to wonder if she would ever learn that words could stand for particular objects.

One morning, her teacher, with the hope of clearing up the mug-milk-water-problem, took Helen out to the pump. Here she had Helen place her mug under the spout *while Helen herself pumped water* into the mug, and at the same time, spelled into Helen's hand the word, w-a-t-e-r. As the cold water splashed over Helen's hand, she recognized *with a flash of insight*, that the manually spelled letters, w-a-t-e-r, possessed the specific meaning of water. This was the first time, apparently, that she had been able to analyze a situation and to perceive a word in its relation to a particular "object" or event. Almost overcome with excitement, she knelt down and pointed to the ground, and its name was likewise spelled into her hand. Then she pointed to the pump, the trellis, to Miss Sullivan. Within a few hours, Helen had added over thirty words to her vocabulary, all of which now possessed specific and concrete meanings.

READING SIXTEEN[1]

THE RÔLE OF FORM IN LEARNING[2]

Editor's Note

Learning and the Eight Organismic Laws.—Professor Guilford's investigation reprinted, here, well illustrates each of the principles of configuration as it applies to the learning process. He asked his subjects to learn various series of memorial material; some of these series possessed a "formal character" and some not. By "formal character" Professor Guilford means configuration; he is using the literal translation of the German word *Gestalt*. In this particular experiment the configurational character of the material consisted of a systematic plan of some sort with which the members (letters, words, nonsense syllables) of the series had been selected and arranged. In other words, each member of the series could take on a certain meaning or significance with reference to the whole. Other series were not arranged in accordance with a definite plan. That these were learned at all depended upon the fact that the subjects could perceive the members in some kind of simple relationship to the whole, such as their position in the series.

The first organismic law, that the whole is more than the sum of its parts, is demonstrated by the fact that the systematic series contained *much more in them* to be perceived than the unsystematic series. In series *A*, for example, not onlv are there the numbers 5, 6, 8, 11 and so on, but also their relation to the whole. The difference between 5 and 6 is 1; between 6 and 8, 2; between 8 and 11, 3; between 11 and 15, 4. The difference changes, thus making it necessary to think of a new number each time that expresses the relationship. But the difference increases in a systematic way down the series. The learner's apprehension of the column as a whole is a conscious field whose field property is exhibited in the awareness of the plan upon which the column is based.

In series *B*, however, there is *much less to be perceived,* only the numbers in their spatial and temporal relations (with the exception of the fact that they are increasing in size through the series). Yet it took practically twice as long for the subjects to learn *B* than to learn *A*. Here, the quality possessed by the perceptual configuration, over and above its parts, is precisely what is meant by *insight.*

Any task is more than the sum of its parts. In order to master it successfully it must be apprehended as a whole and *learned as a whole.* This is one of the facts that was discovered long ago under the influence of association psychology. If the relationships of the parts to the whole are too numerous and complicated to be apprehended by the learner, he is not ready, through lack of maturation—not drill—for so difficult a task. Either the attempt at pacing has been too rapid to fit his rate of maturation or else his maximum level of insight is too low. The latter might readily be the case if the subject was constitutionally dull, and is often the reason why animals fail to learn tasks imposed upon them by human beings.

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[1] Parallels *The Science of Psychology,* Chapter IX or XI.

[2] By J. P. Guilford, University of Nebraska. From the *Journal of Experimental Psychology,* Vol. 10, No. 5, 1927, 415-423. By permission.

The same law holds for the *execution* of tasks. One can never learn golf, first by acquiring separately the proper wrist movements, then the elbow movements, shoulder movements, waist movements and knee movements, finally putting them all together. The necessary co-ordinations of the body do not develop in this fashion. The movement as a whole must be made first. It may be awkward at the outset, but only because it is undifferentiated. The delicate movements must emerge through a differentiation process, which is a phase of maturation. Then, as the finer movements emerge, they are already integrated; they do not require being "put together" by association. Indeed, attempts to "put separate movements together" by association ruins one's golf! *Here again, the quality of the organized movement, as a form of conscious behavior, is insight.*

Presumably, a rat learns a maze in the same way. There are several alleys leading from the starting place, some open and some blind. Certain of these alleys bring the rat to its food. But the rat cannot see over the partitions, and until it has explored the maze sufficiently to obtain an "idea" of it as a whole one alley is as good as another. If, from the standpoint of a human experimenter, the rat makes a great many "false" movements before it perceives the relations of alleys to the goal, they are false only with reference to the experimenter who can see the goal in its relation to the total maze. These movements cannot be regarded as part of the rat's learning process. Nevertheless, the statement is frequently made that one eliminates random movements in acquiring muscular skill. On the other hand, if the goal is perceived only in a general way, as in trying to solve a mechanical puzzle, so-called random movements are made only when the learner *purposely* makes them, and they might just as well never have been made, so far as the learning is concerned, except as by chance they lead to a solution. But *chance will not account for learning.* There is another type of so-called random procedure that is not really a trial and error procedure, but a systematic attempt to reach some goal whose exact position is unknown. Hunting deer or partridge are good examples. Fishing is another. If movements of this type are made in solving puzzles it is not correct to regard them as random, for they are organized movements made with reference to a total situation.

The first organismic principle, then, leads to a very important law of learning, *that it is better to learn by wholes than by parts.* In fact, learning takes place in direct proportion as the conditions of this law are satisfied. It is when this law and the conditions under which it operates are violated, that attempts to learn are characterized by so-called trial and error procedures.

The *law of derived properties,* that parts derive their properties from the whole, is illustrated in Professor Guilford's experiments, in the different properties possessed by similar numbers in different series. In series *A* there is the number 8. It is only with reference to the whole that it has the property of third-in-line; by virtue of a different whole, as in series *L*, it has the property of being tenth-in-line. Again, 8, in series *A*, has the property of being two more than the preceding number, 6, and three more than the following number, 11, and this relationship corresponds in a certain way to all the other number-differences in the column. But in series *L* the properties of 8 are quite different. It is three more than the preceding number, 5, and five more than the following number, 3, in virtue of its membership in a whole where the number-relationships are plus three, then minus five, plus three, then minus five, systematically throughout the series. If the number-relationships were different 8 might not be in the column at all!

Numbers have been described, here, as if they were external objects exclusively. But they are not. They are things *perceived,* and their *properties* are things *perceived,* which means, of course, that the mental operations involved in learning the numbers are really the things we are talking about. Out there on the printed page the numbers are nothing but meaningless ink-blots. Parts of a given learning process, then, as illustrated by the *perceptions* of individual numbers in Professor Guilford's series, derive their properties from the wholes of which they are parts. The wholes are the *apprehensions* of the respective series as organized units.

Again, take the golf illustration. Twisting one's wrists means nothing in its own right. As a part of the golf-learning process it has no properties whatever without reference to the movement that takes place in the body as a whole. It belongs to the elbow movement just as much as 8 belonged to 6 in series *A*, to leg movements as much as 8 belonged to 11, and to the movement-as-a-whole quite as much as 8 belonged to the series as a whole. In a different configuration, with a different organization, the wrist movements might not be present at all, just as 8 would be absent if the series were organized differently.

What, then, are the properties of wrist action in golf? First, they have the particular purpose of giving momentum or snap to the swing of the club. Second, they assist in maintaining the necessary pendulum-character of the swing. But try to put a snap into the swing by twisting the wrists alone, without making an organized body movement, and the result is disastrous; there is neither snap nor swing. This fact can be tested by any one if he will try to execute the act with the rest of his body held immovable either by complete relaxation or by assuming a rigid stance. Thus, the properties of the wrist movements are derived from the movement of the body-as-a-whole.

It would be a simple matter to work out the same principle in connection with the rat in a maze.

The bearing of the *third* organismic law (the law pertaining to *the determination of action*) to the learning process should now be evident. Professor Guilford's subjects could not learn unsystematic series so readily as the systematic, because their perception of the series as a whole was not of a nature to determine effective ways of perceiving the parts. There was not so much of a whole to perceive. Similarly in golf. If the movement as a whole is not perceived there is no way of knowing how to make the necessary balanced movements of the different parts of the body. The proper wrist movements are conditioned by the total body movement. Accordingly, if one kind of stance is taken, the wrists must move in one way; if a different stance is taken, the wrists must move in another way. When the physiological basis of learning is studied, evidence will be available to prove that the nervous system as a whole conditions the activities of its parts, in the process of learning.

The *law of individuation* applies. Professor Guilford's subjects described how their first perception of the plan was vague and general. The details emerged step by step. In golf the refined, nicely balanced co-ordinations of the professional were acquired in exactly the same way, by emergence or differentiation, only in this case the process required years, or at least months, for its consummation.

Fifth, fields evolve as wholes in the learning process. In Professor Guilford's experiment, a systematic series was not learned more rapidly than the others by means of a mere associating of numbers together. The members were all seen at first *as a column,* before individual numbers were recognized. Then they were not fixed (learned) in their proper place in the column until the plan of the whole was evident, or, as Professor Guilford states it, until the series as a whole became *structured.* (The word structured means "formed" and is again a literal translation from the German.) Structured, of course, refers to the perceptual process itself, the unitary process going on by means of which the learner apprehends the series.

The preceding laws explain why this is true. The series originally perceived was an undifferentiated whole. The series ultimately learned was a differentiated whole. In the evolution of the one to the other there was no going from the whole to its parts or *vice versa.* If once the plan was perceived and the learner attempted to isolate any of the numbers and to remember them without reference to the whole, the entire process was halted. Let the reader make for himself the same application to golf or to the rat running a maze.

Sixth, learning is intelligible under the assumptions of *least action.* First, one reason why Professor Guilford's subjects learned at all was the fact that they were under tension toward a goal. These tensions demanded resolution toward the end of reciting the series, perfectly, without copy. This tension exhibited itself in the form of memorizing in which all of the mental processes involved were organized with respect to the goal. The learner stopped repeating the series when the tension subsided, that is, when the goal was reached.

Second, curiously enough, in the systematic series where there was more of a whole to be perceived, learning was rapid and easy. In the other series learning was slow and difficult. The greater rate and ease of learning hinged upon a greater amount of differentiation and organization of parts within the whole. This implies that there were more complicated but more nicely balanced systems of stresses within the nervous system of the learner. This, in turn, means more direct activities of the parts with reference to remote ends. Accordingly, the *parts* of the learning process, consisting of the memorization of *in-*

dividual numbers in their proper position in the series, came about more readily. Third, the memorial processes by which each number took its correct position in the learned series were directed in their course by the total perceptual configuration after the same principle underlying the direction of falling bodies within a gravitation system. In physiological terms, an organized system of neural stresses directed the currents that resulted, individually, in the recitation of the separate numbers in their appropriate order. The memorizing of a series as a whole proceeded toward its end in the most direct way, under the circumstances. The German *Gestalt* psychologists call this end "the closure."

The evidence for the least action is detected, first, in the invariable use of a plan whenever it was perceived. No subject ignored a plan that he discovered; in fact he hunted for plans. Furthermore, he always discarded a poorer for a better plan. *Any procedure that eliminates delay and best decreases the tension toward the goal is immediately carried out.* Plan can have no other meaning, in dynamic terms, than an organization of potentials. Second, evidence for the law is found in the direct relationship between rate of learning and degree of configuration. Sudden action is based upon a demand for sudden release of tension between potentials of considerable difference. Moreover, suddenness of action implies *directness,* and directness has no meaning in any other than a configurational situation.[3]

In motor learning the conditions for least action are more tangible than in mere memorizing, because movements can be seen. The evidence for the law is the inverse ratio of so-called wasteful movements to the immediacy and definiteness of goals. In terms of the law there are no random movements. In golf, one *aims* at the ball, not at a rock three feet away. Every time he fails to hit the ball it is found that the neuromuscular stresses were not right to *direct the swing.*

The same assumptions make intelligible the directional character of movements, for there are differentials of potential that give direction to them. Suppose, now, that the associational theory be considered in this connection. A player stands looking at the golf-ball and has never struck at it before, or at anything else. This is his first movement (there must always be a first directional movement, some time, so this

[3] This is not intended as a process of reasoning from physics to psychology. The assumptions which make the law of least action intelligible arise out of psychological experience, not physical. Potentials and stresses are felt, not seen. The physicist projects them into the physical world if he uses such concepts at all. The author will attempt elsewhere to show how anthropomorphism in physics is a logical necessity if the interpretive dilemmas in which the physicist finds himself are to be resolved. The organismic point of view demands, logically, that physics rest its conceptual organization of facts upon psychology, even upon sociology. The cruder sciences have long looked to the more exact for final explanations. But this is only part of the story of the relationships between the sciences. There is no less reason why the more exact sciences should not, if they are to be complete, look to the cruder ones for explanations. The dependence must go both ways, if it goes one way. Evidence is accumulating, today, that physicists are recognizing this fact. Science looks in the direction of mathematics for instruments of prediction and control, and in the opposite direction for concepts.

is as good an example as any). There has been no experience. How is the player *to know which way to aim?* Grant that he does not know. *If he does not know which way, he does not know any way, because, in order to execute any movement he must execute it in a certain direction.* He does not have to experiment with an infinite number of random movements, neither does the chick when pecking at the first morsel of food, or the spider in weaving its first web, or the infant in making its first eye-movements. The association theory is incapable of coping with the problem of direction. Suppose that the first movement toward a goal is granted, as a reflex. Unless the reflex be regarded as a *configurational response* it will have no direction other than that determined by the anatomical relations of the muscles and bones, when the bones are regarded as levers and the muscles as springs. The scratch-reflex of a dog (reduced to a reflex animal by an operation) is not aimed at the itching spot on the skin; the leg movements merely fan the air. But the intact dog aims its scratching movements toward the itching spot. Suppose, again, that a correct movement be made accidentally and then, according to associational principles, becomes associated with the goal. The dilemma has not been resolved because the act of forming a so-called association does not give direction to the movement; *it already had direction.* Thus the concepts of association and reflex action will in no way answer the question, How do movements obtain their directional character?

The *seventh* organismic law, that an energy system resists being interfered with, is illustrated especially in learning when the learner puts forth a *maximum effort,* or becomes emotionally aroused, when blocked in his progress toward the goal.

The *eighth* law, the *law of configuration,* is illustrated in Professor Guilford's experiment, first, by the fact that his subjects could perceive the plan of arrangement of the systematic series, second, by the fact that when there was no significant plan his subjects *hunted for one.* Moreover, when a plan was lacking they made the series into as good a configuration as they could by perceiving the numbers or syllables in their simple spatial and temporal relationships.

An analysis of the memorizing process as a whole shows that the conditions for any given part of the process were set up in advance by a total situation. The same set of conditions induced the tension and provided the goal. These conditions were the instructions given the learners, and the various ways in which the series had been arranged, and the character of individual members in this series. Then the learning was a unified, continuous process, a temporal configuration which directed the detailed events as they took place. The completed learning was not a concatenation (binding together) of successive fixations of numbers into their proper places in the column, nor a linking of one number to its neighbor by force of repetition.

So in golf; every swing is a movement of the body as a whole in response to a total situation, the situation involving the position, size, and weight of the ball, length and weight of the club, the angle of the club-head, distance of the green from the observer, character of the intervening fairway, and so on. The player can always tell when he is most likely to make a good stroke because he can feel the proper balance of tensions in the whole body and can rehearse the integrated movement in advance. When the pattern cannot adequately be felt something goes wrong with the stroke.

The monumental works of Ebbinghaus, G. E. Müller and their pupils on memory have stood for a long time as the final word on learning and memory regarded at large. The notion that the learning of such material as nonsense syllables is typical of all learning and reproduction is based, however, upon the assumption of atomistic associationism, an assumption now severely questioned. Even a series of nonsense syllables, according to Ogden,[4] possesses some significant unitariness; although, like the conditioned reflex, it is a limiting case of a structured whole. "Memorizing is an activity involving a dynamic configuration of behavior. . . . Indeed, it is only when the series becomes a unified whole, with a certain pictorial composition, with a specific articulatory progression, or with beat, measure and melody, that the series can be memorized at all. . . . What is remembered is always a configuration, the members of which hold together precisely for the reason that they are the members of a configuration."[5]

In this study we shall not try to prove that memorizing would be impossible without some degree of unitariness. Other work may be cited in evidence of this fact.[6] We merely wish to demonstrate the effectiveness of a high degree of formal character in a series which is to be memorized. We have tried to put into a series something analogous to melody. Each series was constructed according to a general principle or scheme. When taken as a whole, it had a meaning or significance which was, if possible, new and unfamiliar.

Material. The following lists were devised for the experiments. Six series of numbers (*A, C, E, G, J, L*) were arranged according to some numerical progression. These will be designated hereafter as the *"Y"* series. Six other series *(B, D, F, H, K, M)*, to be called *"X"* series, were made to correspond to them in length and type without having any particular formal character. And, finally, six series *(N, O, P, Q, R, S)*, which we shall call the *"Z"* series, were constructed according to the plans of the first six but with different items. (See Lists 1 and 2.)

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[4] R. M. Ogden, *Psychology and Education*, 1926, 187 ff.

[5] *Ibid.*, 210.

[6] K. Lewin, Die psychische Tätigkeit bei der Hemmung von Willensvorgängen und das Grundgesetz der Assoziation, *Zsch. f. Psychol.*, 1917, 77, 245 f.; M. Smith & W. McDougall, Some experiments on learning and retention, *Brit. J. Psychol.*, 1920, 10, 204-207.

LIST 1

X AND Y SERIES

A	*B*	*C*	*D*	*E*	*F*	*G*	*H*	*J*	*K*	*L*	*M*
5	2	94	96	2	2	8	7	3	7	13	12
6	3	81	87	5	6	7	12	5	9	16	14
8	7	69	64	3	9	9	1	8	4	11	10
11	10	58	55	6	7	6	5	12	8	14	16
15	19	48	41	4	4	10	11	4	5	9	7
20	24	39	31	7	8	5	14	6	14	12	13
26	29	31	30	5	1	11	2	9	6	7	9
33	32	24	24	8	3	4	8	13	10	10	11
41	48	18	17	6	5	12	6	5	13	5	3
50	58	13	12	9	8	3	10	7	6	8	6
60	65	9	9	7	6	15	3	10	3	3	5
71	72	6	5	10	4	2	9	14	8	6	8
83	88	4	3	8	10	14	13	6	11	1	4
96	91	3	1	11	5	1	4	8	15	4	1
								11	12		
								15	5		

LIST 2

Z SERIES

N	*O*	*P*	*Q*	*R*	*S*
8	97	5	9	5	15
9	84	8	8	7	13
11	72	6	10	10	13
14	61	9	7	14	16
18	51	7	11	6	11
23	42	10	6	8	14
29	34	8	12	11	9
36	27	11	5	15	12
44	21	9	13	7	7
53	16	12	4	9	10
63	12	10	14	12	5
74	9	13	3	16	8
86	7	11	15	8	3
99	6	14	2	10	6
				13	
				17	

The chief problem with the number series was to determine quantitatively the degree of facilitation of learning where form was present and experienced, and to determine how such a form emerges in learning.

Another list was made of other materials, *i.e.,* nonsense syllables with an orderly sequence of letters, series of French or English words, and series of letters, in all of which were concealed English sentences. (See List 3.) This material is so heterogeneous, however, that quantitative results are not significant. The forms were generally too obscure and did not emerge readily, at least during the few repetitions necessary for the learning of a series.

Subjects. Six naïve subjects (Ss) took part in the experiments. They were enrolled in a second course in psychology, but they had had no laboratory experience in the subject.

Procedure. The members of the series were exposed one at a time in a rectangular window, 2 seconds for each item. After each exposure of the series, S attempted to repeat the items. He was urged also to comment fully upon his learning. In this way some notion of the manner in which the forms emerged and of his attitude was usually obtained. A first group of 12 series, including *X* and *Y* series (List 1), was learned in the order, *L K J M H G E F C A D B,* thus alternating the *X* and *Y* series, and giving the *Y* series in the order of decreasing difficulty of form as nearly as could be determined before trial.

The instructions presented to S in printed form were as follows:

> You will be given a series of numbers, one at a time, which I want you to learn. Say each one to yourself as it appears in the window. After each trial I will ask you to repeat as many members as you can, in the correct order if possible. You will also be asked to make any remarks which you can think of in connection with your learning of the series.

The remaining six of the eighteen *X*, *Y* and *Z* series (List 2), which possessed the same forms as those in the first group, were given in the order, *Q R S P O N,* and with the following revised instructions:

> You are to learn the series as before. Each series is constructed according to some principle or scheme, or, taken as a whole, it has some meaning. Watch for the scheme, but at the same time go on memorizing the series in the usual way until it occurs to you.

The series of nonsense syllables, words and letters were mixed in with the number series, half of them under the earlier instructions and the other half under the later. Under the early instructions S had to discover for himself that there was form in the series. There were individual differences in this respect; one S found the intended form in the first *Y* series which he learned, whereas another S found none until the later instructions were given. Once form is found, S is under some degree of self-instruction to look for it in every series.

LIST 3

1	*2*	*3*	*4*	*5*	*6*	*7*	*8*	*9*	*10*
ZAN	FAZ	C	G	que	ou	con	myc	force	thick
YED	DEY	D	Q	ne	des	sta	oun	Korans	wall
XIF	GIX	B	T	beau	ni	nti	try	heaven	it
WOJ	QOW	M	U	dit	beau	nop	rig	ye	tea
VUL	NUV	N	R	qui	dit	lew	hto	sago	of
UAN	MAU	O	N	sabot	qui	ast	rwr	are	myrrh
TEP	ZET	B	G	dis	la	hec	ong	feathers	seize
SIC	BIS	S	4	ni	des	api	sti	brat	knot
ROS	VOR	A	O	de	ci	tal	llm	four	strain
QUV	LUQ	R	I	beau	or	oft	yco	than	debt
PAK	KAP	2	C	dit	pense	het	unt	this	drop
OEZ	PEO	O	U	elle	si	urk	rys	count	path
NIF	XIN	S	R	tra	d'ou	sof	hes	anent	Asa
MOJ	JOM	I	M	la	tel	old	aid	anew	gent
LUP	WUL	C	T					neigh	tell
								shun	reign

Results.—The results by series for all Ss is given in Table I. A number italicized means that the form emerged before the final exposure required for one correct recitation. A number set in blackface means that the same form had emerged before.

TABLE I

Subject	*A*	*B*	*C*	*D*	*E*	*F*	*G*	*H*	*J*	*K*	*L*	*M*	*N*	*O*	*P*	*Q*	*R*	*S*
B	3	2	3	2	4	3	3	3	4	8	5	3	*1*	*1*	*1*	3	*2*	3
H	*1*	3	2	3	*1*	3	*2*	5	*1*	9	5	2	1	*1*	1	1	2	2
L	2	3	4	2	*1*	4	4	3	13	6	7	3	4	7	1	1	***3***	***2***
Mc	*1*	5	10	4	*1*	4	*1*	5	*3*	13	*3*	3	*1*	*2*	1	1	1	1
M	*1*	3	3	3	*1*	4	*2*	3	6	8	10	3	1	*2*	1	2	*1*	*1*
S	***2***	3	3	3	*1*	3	***2***	3	***3***	4	5	4	1	*2*	1	2	1	*1*

We may ask several questions about these results. (1) Does the emergence of form facilitate learning to any great degree? (2) Does failure to emerge when it is expected interfere with learning? (3) Is there a transfer of learning through the learning of certain forms?

The last two questions arise from considerations falling outside our problem and from casual observations made during the experiments. Expectancy, as pointed out by Ogden,[7] is one of the important conditions for learning, and one which has been often overlooked. Expectancy sets an end which requires fulfillment, and if this end is not fulfilled in some way the result is dismay. The comments of our Ss satisfy this general description in all cases where an expected form did not emerge. Will quantitative results tell the same story? According to the same author, an experienced configuration is the secret of transfer of training.[8] The form, like a melody, is transposable, and may be clothed in new members. Will the forms residing in our number series be transposable in the same way?

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[7] *Op. cit.*, 248.

We can answer the first question by comparing the results from the *X* series with those from the *Y*. Those from the *Y* series must be subdivided into the cases in which the form emerged and those in which it did not. These cases will be known as *Y*1 and *Y*2 respectively. If form is an aid, the average number of trials in learning an *X* series should be greater than that in learning a *Y*1 series. Again, the number of trials in learning a *Y*2 should be greater than in learning a *Y*1 series. This is true for every S and also for the combined averages.

TABLE II

Subject	*X*	*Y1*	*Y2*	*Z1*	*Z2*	*Z3*
B	2.6	...	3.6	...	1.0	3.0
H	3.2	1.3	3.5	1.2	1.0	...
L	3.0	1.0	4.2	1.0	2.0	5.5
Mc	4.2	1.5	10.4	1.0	1.5	...
M	3.2	1.3	6.5	1.3	1.5	...
S	3.2	1.7	4.0	1.3	1.5	...
All (av.)	3.2	1.4	4.6	1.2	1.4	4.25
Cases	30	16	14	15	11	4
P.E.$_{av.}$	.11	.12	.39	.07	.07	.55

The results from the *Z* series, in Table II, are subdivided as follows; *Z*1, where the form emerged for the second time; *Z*2, where the form emerged only once; and *Z*3, where the form did not emerge at all.

When an expected form fails to emerge, the interference with learning is indicated in two ways. The averages in the *Z*3 series were greater than those in the *X* series, and even those in *Y*2 were greater than those in *X;* although the *Y*2 group contained several cases where form was not yet expected or known to be present. This result agrees with numerous comments of the Ss to the effect that when looking for form they failed to learn the items of the series.

Transfer through form is strongly evident in several ways. The results for *Y*1 are usually greater than those for *Z*1; a form is more quickly learned the second time. Results for *Z*2 are also usually greater than those for $Z1$. The importance of the form over the members of the series was shown in the many cases of recitation during which the S was doubtful about certain items, or else did not know them at all except through the demands of the scheme.

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[8] *Ibid.*, 258 ff., 279 f.

On one occasion an S declared that he had learned a certain Z series before, whereas the members were entirely new. Often a correct recital was prevented merely by the fact that S failed to notice the beginning and final members of the series. Many of the forms were continuous, *i.e.,* had no "natural" beginning or ending, were not "closed" forms; they were lacking in precision at the ends except for that precision which belongs to any series and which usually permits an S to retain the first and last members easier than the middle members.

We may ask whether implicit form aids learning; whether the Y series are more readily learned than the X even if S does not experience the intended form. If this were true, we might expect the results in the $Y2$ series to be less than those in the X series, but such is not the case. We have already pointed out that the delayed learning in the $Y2$ series was due to interference occasioned by the failure to find a form where one was expected.

If we take only those $Y2$ series, nine cases in all, which were learned before the Ss had discovered any form at all, and compare them with the corresponding X series, the difference is still in favor of the X series; 3.0 trials for the X series, and 3.6 trials for the $Y2$ series. If a conclusion is warranted from so small a number of cases it must be that implicit form has no influence on learning. In order to be effective in learning, a form must be somehow represented as such in the experience of the individual.

If we combine the results for the six Ss, thus neglecting individual differences in order to get more cases, statistical tests for significant differences can be made (Table III).

Here the first three differences are well above statistical certainty, confirming the previous conclusion that the emergence of form always facilitates learning. The next two differences are almost certain to be universal, demonstrating again that the failure of form to emerge, when it is expected, delays learning. The evidence for transfer through form is not nearly so certain; although there is a degree of probability in its favor.

One thing which undoubtedly worked against the effect of transfer was that a different form can emerge in the same series upon a second appearance. Series *G*, for example, may be conceived either in pairs of numbers diverging each time by one unit, or it can appear as two alternating series, one ascending, the other descending, or as a series with increasing differences between successive members, alternating in sign. Transfer by way of form should take place only when the same form arises on a later occasion. Our own cases in which a new form arose on the second occasion were so rare, however, that no quantitative comparisons are warranted.

TABLE III

Series	*Diff.*	*P.E.*$_D$	*D/P.E.*$_D$	*P.C.*
*X–Y*1	1.8	.17	10.59	1.000
*X–Z*1	2.0	.13	15.39	1.000
*X–Z*2	1.8	.13	13.92	1.000
*Y*2*–X*	1.4	.40	3.50	.982
*Z*3*–X*	1.45	.57	2.55	.914
*Z*2*–Z*1	.2	.10	2.00	.823
*Y*1*–Z*1	.2	.14	1.43	.665

What can we say qualitatively concerning the way in which form emerges under the conditions of these experiments? It must be mentioned again that the Ss were naïve and unused to introspective methods. Certain deductions can be made from their comments, however, which are quite full. We may ask first of all whether the forms emerge suddenly, all at once, or gradually and continuously, or step by step. Do they come piecemeal, by way of an association of elements? or do they come by way of a general outline, the detailed structure of which is later filled in?

They do not appear to come all at once completely structured in their final form unless the pattern is extremely simple or familiar. Neither do they appear to come by way of a continuous growth; but they do come step by step. The kind of step varies with the kind of materials, and with the difficulty of the form. On the whole, the forms in the number series emerge first of all as a generalized pattern. We can perhaps distinguish the following steps, which are more apparent in the more difficult series.

1. A feeling of regularity or order, as attested by the following comments:

> "There were glimmerings of a scheme all the way through this but I couldn't get it precisely" [Series *G,* after the first trial]. "There was such regularity in that one that I did not notice the numbers well enough" [Series *G,* after the first trial].

And yet, in these instances, the Ss were unable to describe any scheme.

It is questionable, however, whether this feeling of regularity is a universal or necessary prelude to the process. A feeling of regularity or order might occur in the learning of an irregular or orderless series. This question should be tested with a large number of series with and without formal character to see whether the feeling is "illusory" or dependent upon a realizable order.

2. Awareness of direction.

> "I started looking for a scheme and saw none except that they were arranged in order of descending quantity" [Series *C*, first trial]. "The series is going down all the time" [Series *D,* first trial].

3. Specific nature of direction.

> "I noticed a natural sequence, a number in each ten's place from 30 to 90" [Series *A,* third trial]. "I noticed that the numbers are coming in a regular progression; they go up; there are four of them" [Series *J*, second trial]. "Noticed a scheme of four numbers, beginning with a digit and increasing and then dropping again and so on" [Series *R,* first trial].

4. Specific relationship of members.

> "I noticed more relationship this time. The second number [in each group of four, series *R*] is two more than the first; the third is three more than the second; the fourth is four more than the third" [Second trial]. "There is a difference of three in each pair, starting with 2 and going up to 8" [Series *E,* first trial].

Not all series require all these discrete steps, for the whole process often occurs during one repetition. On the other hand, some of the steps may be carried through more than one repetition. We have merely tried to picture the process of emergence as it can be pieced together from scattered comments. The chief thing to be noted is the direction of the process, more general forms coming first, with a more detailed structure coming later. If this analysis be accepted, the form does not arise from the piecing together by association of discrete elements and relations. The elements become merely the necessary members of the complete form and are remembered as such.

The rôle of relations in the learning process is not clear. By relations we mean the relationships of each member to its near neighbors in the series. There is a tendency, especially after S has discovered that forms of a certain kind are present in some of the series, for him to build up the scheme by induction. He notes the relation of each member to the next, watches for repetitions of these relationships and gets the whole scheme from these "samples." It was a frequent occurrence for S to report that he got the scheme from the first three or four numbers in the series without having to see the whole series. We may say, however, that the relationships themselves are forms, over and above their elements, and that in so far as S projects them into a tentative scheme for the whole series they are, by virtue of imagination, the unifying form of the whole series. This tentative form would be discarded for another as new-appearing items fail to fit the scheme. The relations would therefore be secondary to the total scheme once the latter has emerged. This is probably the reason why S finds it difficult to verbalise the scheme; although it is the relations which he has to put into words in order to describe the form.

In the other series, the process seems to be somewhat different. The series of nonsense syllables and of words possess forms which did not readily emerge; consequently, data from these series are very meager. As for the series of nonsense syllables we can say that the two principles, the order of initial, or final, consonants and that of the middle vowels, came independently and all at once. In the series of words which contain sentences, the form was built up step by step. The following case is typical (Series 14).

> "I begin to see something now. It must be 'the quality'" [after the first trial]. "I get 'as a gentle rain' at the end" [after the fifth trial]. "I get it all now" [after the sixth trial].

In conclusion, we may say that a highly unitary form in a series of numbers, even though unfamiliar, greatly facilitates learning, and that the failure of form to emerge, when one of a certain kind is expected, interferes with memorizing. Form takes precedence over the members of the series and their interrelations, and once experienced it emerges more readily in a series possessing new members. Under the conditions of our experiment, the forms in series of numbers emerged step by step, and in direction from a general, outlined form to a more particular, structured form; although form may also come by way of successive inductions based upon the interrelations of the members. It is our notion that the material and the methods of learning used here approximate more nearly the normal case of human learning than do series of nonsense materials learned by sheer force of repetition.

READING SEVENTEEN[1]

TRANSFER OF LEARNING[2]

Editor's Note

An Organismic Theory of Transfer.—The problem of transfer of training holds an important place in the history of experimental work on learning. This problem dates back several decades to a time when "formal discipline" was a live issue in educational circles. Does the mastery of one subject, such as mathematics, assist the student in learning another subject such as a foreign language? As Dr. Bray has pointed out in the following article,[3] the results of extended researches on transfer, under various types of conditions, reduce to the conclusion that where there are common methods of work and common contents in the material to be learned, transfer takes place. This conclusion has led to the theory of identical or common elements, that is, common methods and contents were interpreted as elements or parts *possessed in common* by two or more learning processes. Thus, when a learner shifts from one task to another he finds certain elements in the new task, the common elements, already learned. This decreases the time and effort required in mastering the new task.

It is very evident, however, that this associationistic and atomistic conception of transfer has obscured to investigators the real nature of the process. Evidence of this fact is found in the prevailing conception that there are two kinds of transfer, one in which the learner is required to shift from one type of subject matter to another, say English to Psychology, or from multiplication to division in arithmetic, and one in which the learner is required to shift from the execution of a task with one part of the body to the execution of the same task with another part. There is more evidence of transfer in the latter case than in the former, but experimenters who are chiefly interested in the former type are not inclined to regard the latter as a form of transfer. But in reality the difference seems to be only one of degree, and the facts pertaining to the latter type seem clearly to reveal the nature of the process. Transfer is not dependent upon common elements because there are no elements from the standpoint of organismic psychology. Rather, it is a case falling under the third organismic principle, namely, that the whole governs the activities of its parts. In the act of learning a given motor task with one hand there maturates an organized neural system of stresses that involves the brain as a whole, so that all parts of the body, anatomically suited to execute the task, can be used thereafter. Stated in different words, the learning is a configurational process involving insight into the total situation. Insofar as that situation is not altered by the conditions of transfer, the insight is sufficient to guarantee continued performance. When the transfer involves the development of a new type of configuration the performance suffers. Any other loss observed in the transfer depends upon limitations imposed by the anatomy of the part of the body to which the transfer is made.

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[1] Parallels *The Science of Psychology,* Chapter XI.

[2] By Charles W. Bray, Princeton University. From the *Journal of Experimental Psychology,* Vol. 11, No. 6, 1928, 443-467. By permission.

[3] This study was carried out at the Psychological Laboratory of Princeton University.

The slight impairment of the learning process found by Dr. Bray in transferring from the hand to the foot depends quite possibly upon anatomical conditions. The muscles of the leg are larger than those of the arm, and they are not attached to the bones in exactly the same fashion. It is true that these anatomical limitations can be overcome remarkably well with maturation, for persons deprived of their arms learn to write, sew, crochet and throw marbles with their feet. Nevertheless, in order to overcome mechanical difficulties, there is required the apprehension of relations of the moving part to the goal, that were not present before the transfer takes place.

This theory is borne out by Dr. Bray's discovery that 10 trials with the hand lead to practically all the transfer that can be effected in shifting from the hand to the foot. The improvement that takes place in the hand after the tenth trial involves those differentiated aspects of co-ordination that cannot be transferred to the foot because of anatomical and therefore mechanical differences. These refined aspects of co-ordination of both the hand and the foot must emerge separately from the total neuromuscular pattern.

A similar explanation might account for the loss found in transferring from the right to the left hand. In the less used hand and arm the muscles are probably not so strong. Moreover, the spatial relationships between the right hand and the body are reversed from those of the left. For example, if a person were to write with his right hand, movements are *away* from the body. If he were to write with his left hand, movements are toward the body. This means that the two movements are "mechanically" different. The same situation obtains in the two hands alone. The thumbs and fingers are reversed with respect to right and left. Thus when the learner holds a pencil in the left hand the grasp is not the same, and movements required for writing are reversed. This fact can be demonstrated by writing the same words, mirror-fashion, with the left hand, that the right hand is writing. With a little care this can be done without practice.

The theory of transfer suggested, here, is borne out further by Dr. Bray's results. There were certain subjects who showed little improvement after transfer was made from the hand to the foot. These same subjects had made relatively little original progress with the hand.

Returning now to the problem of transfer from one subject matter to another, the nature of the transfer is indicated by the results of such experiments as Dr. Bray's. The process of learning multiplication is a configurational one, involving the brain as a whole. To the extent that additional insight is involved, because new interrelationships between numbers are to be discovered, there is no transfer from multiplication to division. The total pattern involved in multiplication is still used, but the perceptions of new and different number-inter-relationships involved in division must emerge out of this pattern, just as the more refined co-ordinations in the leg must emerge separately out of the total neuromuscular pattern employed in using the hand. In the organismic conception, common elements do not shift from one pattern to another. Strictly speaking there is no transfer except as the learner shifts from one situation to another. The same total pattern is used before and after the "transfer," but insofar as the conditions that support the total alignment of stresses are changed, new parts must emerge after the "transfer" is made. This is not altogether out of harmony with the theory of generalization mentioned by Dr. Bray, but is incompatible with the theory of identical elements.

One more interesting problem is raised by experiments on transfer. This problem stands out with a striking clearness in Lashley's study with the monkey. The fact of "transfer" is incompatible with the theory that learning depends upon the lowering of synaptic resistances. Habits are not formed by burrowing pathways through the nervous system.

It has long been known that practice of one part of the body in performing a skilled act increases the ability of the bilaterally symmetrical part in the same act. The effects of practice are transferred from one hand or one arm to the other, and from one toe to the similar toe of the other foot. This phenomenon has been called cross-education.[4] There are also indications that a similar transfer takes place to all parts of the body, not merely to bilaterally symmetrical parts. The experiments to be reported here were designed to study the transfer of learning from one limb to any other.

Bilateral transfer or cross-education was first noticed by Weber. In a letter to Fechner[5] he described the activities of his son, who was a spontaneous mirror writer, using his left hand in reversed fashion although he had been taught to write only with the right hand. Weber concluded that in training the right hand, the left had been trained simultaneously to make symmetrical movements. Fechner reported a similar observation on himself. In the course of one of his investigations, he had frequent occasion to write the figure 9 with his left hand; then he found that when he used the right hand again he reversed the figure mirrorwise. A related phenomenon was discovered at the same time by Volkmann.[6] In an investigation on the two-point limen, he found that practicing the left arm in the discrimination of two points helped the right arm to make the discrimination. Further work showed that practice effects were transferred to symmetrical parts and to parts related in function but not to the rest of the body. These observations were later extended by numerous investigations into the subject. Scripture, Smith and Brown (pp. 114-119) devised an experiment to show transfer from right to left hand of the ability to thrust a needle into a small hole without touching the sides of the hole. They first made a test of the left hand in this act and then practiced the right hand. A re-test of the ability of the left hand showed that it had improved 26 per cent. In the same way, they found that practice by the right hand in pressing a dynamometer resulted in a gain of strength of the left hand. Their method has been followed in numerous later experiments. It consists in making two tests of some part of the body and interposing a period of practice by some other part. The difference between the first and last tests is assumed to show the transferred improvement. Unfortunately the method fails to consider the effect of the first test upon the second. Acts of skill are known to improve greatly in the first few trials; so it is only to be expected that the second test will show an improvement over the first. For this reason the method described above gives no adequate measure of transfer. This defect was pointed out by P. H. Ewert,[7] who has shown that early estimates of the amount of transfer must be considerably discounted, and that a different method must be followed.

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[4] E. W. Scripture, T. L. Smith and E. M. Brown, On the education of muscular control and power, *Stud. Yale Psychol. Lab.*, 1894, 2, 115.

[5] G. T. Fechner, Beobachtungen, welche zu beweisen scheinen, dass durch die Uebung der Glieder der einen Seite die der andern zugleich mitgeübt werden, *Ber. d. sächs. Gesell. d. Wiss., math. phys. Cl.*, 1858, 10, 70. (Reviewed by W. W. Davis, Researches in cross-education, *Stud. Yale Psychol. Lab.*, 1898, 6, 6.)

The defect in the method does not, however, completely invalidate the early work on transfer. Frequently it was found that the unpracticed part gained on its second trial as much as the practiced part gained in many trials. Thus in the work of Scripture, Smith and Brown it was shown that the effect of ten days' practice with the right hand caused an improvement in the left equal to the improvement shown in four days by the right. This seems fair evidence for the existence of transfer, although no measure of the amount can be so gained.

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[6] A. W. Volkmann, Ueber den Einfluss der Uebung auf das Erkennen raümlicher Distanzen, *Ber. d. sächs. Gesell. d. Wiss., math. phys. Cl.*, 1858, 10, 38. (Reviewed by Davis, *op. cit.*)

[7] P. H. Ewert, Bilateral transfer in mirror drawing, *Ped. Sem.*, 1926, 33, 235-249.

Following Scripture numerous experiments have been devised to show cross-education in skill, sensory education, fatigue and strength. In skill, Davis [8] found that practice of the right great toe in speed of tapping was shared to some extent by the left great toe and that accuracy in hitting at a mark with a fencer's foil was transferred from right to left arms. Woodworth [9] has shown that training in making accurate movements transfers from left hand to right when the training is successful. Unsuccessful training has no cross-effect. Other studies have shown cross-education in mirror drawing,[10] in juggling,[11] in ability to use a typewriter,[12] and, finally, in ability to set numbers in an adding machine.[13] In the field of sensory education several studies have been made of transfer. Dresslar [14] has repeated and confirmed the work of Volkmann on transfer in tactual discrimination. Experiments on the conditioned reflex show a similar phenomenon. Anrep [15] has found that scratch stimuli on either side of the body are equivalent in calling out salivary secretion; although only one side of the body has been used in establishing the conditioned reflex. Cross-effects in sensory processes do not always occur, however, for one author [16] finds that adaptation to weights is not transferred from one hand to the other.

Experiments on the transfer of fatigue give somewhat ambiguous results, for cross-effects can be both positive and negative. Bryan,[17] by testing the rate of tapping from various joints, found crossed fatigue effects, independent of any general bodily fatigue. Peculiarly enough, however, one subject showed an opposite cross effect, that is, the fatigue of one joint caused an increase in the rate of the same joint of the other side. This result is confirmed by Joteyko,[18] who likewise finds positive and negative cross-effects in fatigue. The nature of the transfer seems to be constant in any one individual.

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[8] W. W. Davis, Researches in cross-education, *Stud. Yale Psychol. Lab.*, 1898, 6, 6-50.

[9] R. S. Woodworth, The accuracy of voluntary movement, *Psychol. Rev. Monog. Suppl.*, 1899, 3 (no. 13), 105 f.

[10] D. Starch, A demonstration of the trial and error method of learning, *Psychol. Bull.*, 1910, 7, 20-23; L. L. Henderson, The psychology of learning (Bachelor's Thesis, 1906; deposited in the Library, Univ. of Wisconsin); H. S. Hill, Minor studies in learning and relearning, *J. Educ. Psychol.*, 1914, 5, 375-386; P. H. Ewert, *op. cit.*, 235-249.

[11] E. J. Swift, Studies in the psychology and physiology of learning, *Amer. J. Psychol.*, 1903, 14, 201-220.

[12] W. Frankfurther, Arbeitsversuche an der Schreibmaschine, *Psychol. Arb.*, 1914, 6, 419; A. Langelüddecke, Untersuchungen über die Vorgänge beim Erlernen des Maschinesschreibens unter normalen und krankhaften Bedingungen, *Psychol. Arb.*, 1923-25, 8, 304-414.

[13] W. H. Norcross, Experiments on the transfer of training, *J. Comp. Psychol.*, 1921, 1, 317-364.

[14] F. B. Dresslar, Studies in touch, *Amer. J. Psychol.*, 1894, 6, 324-330.

[15] G. V. Anrep, Irradiation of conditioned reflexes, *Proc. Roy. Soc. Lond.*, 1922-23, 94B, 409.

[16] L. Steffens, Ueber die motorische Einstellung, *Zsch. f. Psychol. u. Physiol. d. Sinnesorg.*, 1900, 23, 241-265.

[17] W. L. Bryan, Voluntary motor ability, *Amer. J. Psychol.*, 1892-93, 5, 149.

Transfer of strength has been shown by Davis who found that the left arm gained in girth, strength (as measured by a dynamometer) and endurance by exercise of the right arm in swinging a dumbbell, pressing a dynamometer or pulling an ergograph.[19] Similar results are shown by Wissler and Richardson.[20]

The defect in method as noted above has been remedied by Ewert in mirror drawing. Ewert used two groups of subjects, one of which followed the usual procedure. The other group acted as controls, receiving only the end-tests of the unpracticed part. Comparison of the relative improvement of the two groups gives an adequate measure of transfer. Ewert found that the non-preferred hand improved 36 per cent. in time and 21 per cent. in errors by fifty trials practice with the preferred hand. Measured in the old way the transfer would have been estimated at 82 per cent. in time and 76 per cent. in errors. This shows that previous estimates of transfer have been too high, but that some transfer actually occurs. Ewert was also able to show that transfer is greater from the preferred to the non-preferred hand than in the opposite direction.[21]

There are some indications that transfer takes place to parts other than those bilaterally symmetrical. As mentioned above, in the two-point limen Volkmann found that parts related in function shared in the practice of one finger; *i.e.*, other fingers shared in the practice but the arm did not. Other investigators have obtained similar results. Davis[22] found that practice in speed of tapping the right great toe was shared by the index fingers of both hands. The left index finger gained almost as much as the part practiced while the right showed a slightly smaller improvement. The validity of this distinction between fingers is questionable, for Davis had no control-subjects and the difference was small.

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[18] J. Joteyko, Participation des centres nerveux dans les phénomènes de fatigue musculaire, *Année psychol.*, 1900, 7, 161-165.

[19] W. W. Davis, Researches in cross-education, *Stud. Yale Psychol. Lab.*, 1898, 6, 18-29; 1900, 8, 64-109.

[20] C. Wissler & W. W. Richardson, Diffusion of the motor impulse, *Psychol. Rev.*, 1900, 7, 34.

[21] *Ped. Sem.*, 1926, 33, 235-249.

[22] W. W. Davis, Researches in cross-education, *Stud. Yale Psychol. Lab.*, 1898, 6, 6-50.

Lashley[23] also noted transfer-effects in the course of work on cerebral function in learning. He taught monkeys to open a problem box with the right hand. He then destroyed the motor area of the left cortex, thus paralyzing the right hand. When the animals were again placed in front of the problem box, they used the left hand, the feet and sometimes the head to open it. The animals were said to show little or no random movement in using these previously unpracticed parts. In one case transfer to the left arm occurred although it had been temporarily paralyzed during learning by the right.

Theory of Transfer. Several types of explanation have been proposed to account for transfer from one part of the body to others. The early investigators found an explanation in terms of the improvement of some faculty.

Davis, for example, concluded that training the attention and will-power explained in part the transfer of ability to tap rapidly (p. 49). Scripture[24] finds that training the will-power explains crossed gains in strength. Explanation by faculties is no longer acceptable in psychology, for it is a purely verbal explanation. Furthermore, investigations of the doctrine of formal discipline militate against the faculty explanation.

Other explanations of cross-education and transfer from one part of the body to others have been drawn from the related field of transfer from one situation to another. The voluminous work on this subject has led to two theories, that of *identical elements* and that of *generalization,* both of which seem applicable to the type of transfer considered here. The theory of identical elements would say that transfer from one part of the body to another takes place where the subject learns part-activities involved in the performances of both members. Thus Woodworth[25] suggests that common head, eye or trunk movements may be involved in the performance of an act of skill or strength by either hand.

The theory of generalization does not differ greatly from that of identical elements except that it emphasizes conscious rather than behavioral factors. It suggests that transfer is due to the generalization of methods, attitudes or ideals learned in practice by one part of the body and applied to the performance of the act by other parts. This type of explanation is used by Swift [26] in his work on transfer of ability to juggle two balls. He found that methods of handling the balls, of throwing them, and of recovering from bad throws were developed by one hand and then carried over to the other.

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[23] K. S. Lashley, Studies of cerebral function in learning, *Arch. Neur. & Psychiat.*, 1924, 12, 249-276.

[24] E. W. Scripture, Recent investigations at the Yale Laboratory, *Psychol. Rev.*, 1899, 6, 165.

[25] R. S. Woodworth, *Le mouvement,* Paris, 1903, 325.

The two theories are more or less complementary. They can therefore be combined and called the theory of *common elements*.[27] There seems to have been only one effort to isolate experimentally common elements of method or behavior and to determine to what extent they play a part in transfer. Norcross, in his work on bilateral transfer of ability to place numbers in an adding machine, tried to isolate common elements, such as ability to read quickly and accurately the numbers to be listed, familiarity with the keyboard, actual use of the keyboard apart from the setting of the experiment, etc. He found that practice with any of these part processes alone acted favorably on ability to perform the act as a whole. Unfortunately Norcross [28] did not use many subjects. As his data show a large individual variability, his results are not necessarily conclusive evidence for the theory of identical elements. It is further to be regretted that he used no control subjects, for the gains reported (from 7 per cent. to 48 per cent.) might easily be due to practice rather than to transfer.

One further possibility has been advanced to explain transfer. Davis suggests (p. 49) that there may be some actual training of the so-called unpracticed part. This might take place, he suggests, by a spread of the nervous impulse to the motor centers governing the unused part, or by spread of the impulse to the muscles themselves, so that the apparently unused part might actually receive practice. The same possibility is advanced by Woodworth.

There is a wide variety of evidence showing the close relationship between the parts of the body, especially between the two sides. It is known, for example, that injuries to the left side of the cortex disturb skilled acts performed by either side of the body.[29] And in cases of hemiplegia a paralyzed arm may be made to move by movement of the normal arm. These movements are frequently symmetrical to those of the normal arm.[30] Observations of this sort show the close relationship between different parts of the body. There has been no evidence advanced, however, to show that associated movements of the so-called unused part are of any importance in transfer.

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[26] E. J. Swift, Studies in the psychology and physiology of learning, *Amer. J. Psychol.*, 1903, 14, 201-220.

[27] J. V. Breitwieser, *Psychological Education*, N. Y., 1926, 164.

[28] W. H. Norcross, *op. cit.*

To summarize:—There are current two theories of transfer: the theory of common elements and the theory which assumes some sort of practice of the apparently unused member. It is one of the aims of this investigation to furnish experimental data, which may throw light on the relative validity of these two theories.

THE EXPERIMENTS

In order to study transfer from one part of the body to others, mirror drawing was chosen as a task. The procedure for the hands followed that described by Dearborn[31] and used by Starch and Ewert. The subject traces the outline of a star which he can see only through a mirror. An effort was made so to adapt the experiment that the task could be performed by the feet as well as by the hands. The technical difficulties as well as the difficulty of the task prevented the attempt to obtain transfer from hands to feet; but the impression of the experimenter and that of the subjects as well was that the feet gained considerably from the practice of the hands. Transfer from right to left hand was shown by the results; but because of certain errors in the procedure this work was unsatisfactory. The results confirmed, in general, those of Ewert on transfer from preferred to non-preferred hand.

It seemed to be desirable to obtain a method by which transfer from hand to foot could be adequately studied. For this purpose a new experiment was undertaken which involved a combination of two old experiments on the acquisition of skill, namely a mirror-experiment and an accuracy-experiment. The task set the subjects was to hit at a target seen indirectly through a mirror.

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[29] H. Liepmann, Die linke Hemisphäre und das Handeln, *Münch. med. Woch.*, 1905, 48. (Reviewed in *J. f. Psychol. u. Neur.*, 1906, 7, 190.)

[30] F. M. R. Walsche, On certain tonic or postural reflexes in hemiplegia with special reference to the so-called associated movements, *Brain*, 1923, 46, 136.

[31] W. F. Dearborn, Experiments in learning, *J. Educ. Psychol.*, 1910, 1, 374-378.

The subject sat at a table placed against a wall. Mounted on the wall was the target, consisting of a cross drawn on a sheet of white paper, 8x11 inches in size. Direct view of the target was prevented by a wooden screen, set above the table and directly in front of the subject. At the subject's left was a mirror placed at right angles to the wall. By means of the mirror an indirect view of the target could be obtained. The subject, holding a pencil in his hand, placed his arm under the screen with his elbow resting on a spot marked on the table. This spot was directly in front of the center of the cross. The arm was held upright and in such a position that the back of the pencil touched the screen. The subject, looking into the mirror, hit at the target, with the purpose of coming as close to the center of the cross as possible. After hitting he brought his arm back to the starting position, so that the pencil again touched the screen. Then he hit again at the target, trying to correct any previous error. The arm was again brought back to the starting position and the procedure repeated. The speed of hitting was controlled by a metronome beating at 72 per minute. On one beat the subject hit forward, on the next he returned to the starting position, and so on. By having the subject hit in time to the metronome and by having him return to the screen after every hit, both the speed and the amplitude of hitting were controlled. After ten successive hits the subject was told to stop and was given a rest period of two minutes. A new target was then placed on the wall and the procedure repeated.

For practice with the foot a similar procedure was used. The subject sat in an elevated chair with his thigh in a rest. This rest was directly in front of the target, so that the foot hung in front of the mark. The target was obscured by a screen, but on the left was a mirror in which it could be seen indirectly. A pencil was securely attached to a light block of wood, which was strapped firmly to the subject's foot. By this means the subject could hit at the target and leave a mark on it. Speed and amplitude of hitting were controlled in the same manner as above. Typewritten instructions were given to the subjects.

"The object of this experiment is to hit several times in succession at the center of the cross you see in the mirror. The hitting is to be done in time to the metronome, which beats one stroke for the forward movement and one for the backward. After the forward movement return always until you hit the board in back of your hand (foot). In hitting try not to hesitate but hit with one single jab.

"After a certain number of trials you will be told to stop and there will be a short intermission. During each of these intermissions please give a report of your experience during the test, noting methods, if any are used, confusion due to the necessity for timing the hits, or anything important to the experiment. Try not to let this report interfere with your attention to the experiment, however. Make it retrospective. Remember particularly these points:—

"Each hit is to be a single jab at the mark.

"Keep in time to the metronome.

"Hit the board in back of your hand on the backstroke."

The experimenter had at his disposal the sheets of paper on each of which were ten pencil marks representing the subject's efforts for that trial. Each set of ten hits was called a trial. The average distance of the ten hits from the center of the cross gave a measure of the subject's accuracy on that trial. This distance was obtained by laying the sheets of paper over a sheet of graph paper, ruled in 2-mm. squares and illuminated from below. The distance of each hit could be determined by counting the number of squares between it and the target, in the horizontal direction. By means of the graph paper a convenient and accurate method of measuring the performance of the subject was obtained.

It was decided to eliminate the preliminary test of the unpracticed member, as a test of this sort makes it impossible to determine the effect of transferred practice on the first trial with the unpracticed part. It was furthermore desirable to determine the effect of transferred practice on later trials with the new part. Consequently a measure of transfer was obtained by using two groups of subjects, one of which received practice with one part of the body, followed by practice with another part, while the other group received only practice with the latter. In the preliminary experiment undertaken here, one group of four subjects received 60 trials on the right hand followed by 40 trials with the right foot. The other group of four subjects received 40 trials with the right foot. Comparing the records of the two groups on the foot gave a measure of the effect of the hand-practice on the performance of the foot.

A considerable difference between the two groups was found. The effect of the previous practice with the hand was to decrease considerably the amount of the initial error of the transfer subjects. At the beginning of practice there was a large difference between the two groups. This difference decreased considerably with successive practice. Because of the small size of the two groups no definite conclusions could be drawn; but it was felt that an adequate method of studying transfer had been found. For further work it was thought best, however, to simplify the experiment by making the target a simple vertical line instead of a cross. The mirror reverses in the horizontal direction only and to study the transfer of a mirror co-ordination it is best to limit the practice to that form of co-ordination alone.

The target, then, was changed to a simple vertical line, bisecting the sheet of paper. The instructions were changed to meet this new condition. They also emphasized more strongly the necessity for accuracy and cautioned the subjects against practicing between experimental periods or talking about the experiment with other subjects.

Transfer from Hand to Foot

Two new groups of subjects were obtained for the changed experimental conditions. Group I (14 Ss) was given sixty trials practice with the right hand followed by forty trials practice with the right foot. Ten trials were given in one sitting and with a few exceptions sittings were given on successive days. Group II (14 Ss) received forty trials with the right foot. It should be mentioned that in these, as in all groups to follow, only right-handed subjects were used.

The average foot-records of the two groups are compared in Figure 14. They reveal a typical learning curve for Group II, in this case the control group. There is a high initial error, which decreases fairly rapidly in the first few trials, but with slow improvement after the first ten trials. Group I, the transfer group, shows a very different curve; a small initial error, a large fall from the first to the second trials and a period of slow improvement thereafter. The curves of the two groups meet and remain the same after the tenth trial. The initial error of the transfer group is approximately equivalent to that made by the control group on the sixth trial. The transfer group, therefore, has saved the equivalent of about six trials by reason of its previous practice with the hand.

The differences between the two groups are statistically significant. It was found that the difference on the first trial was over seven times its P.E. For the first four trials the differences are more than four times their probable errors, indicating that the differences on these trials are not due to chance. After the fourth trial the differences are less than four times their probable errors, the statistical significance decreasing with the size of the difference.

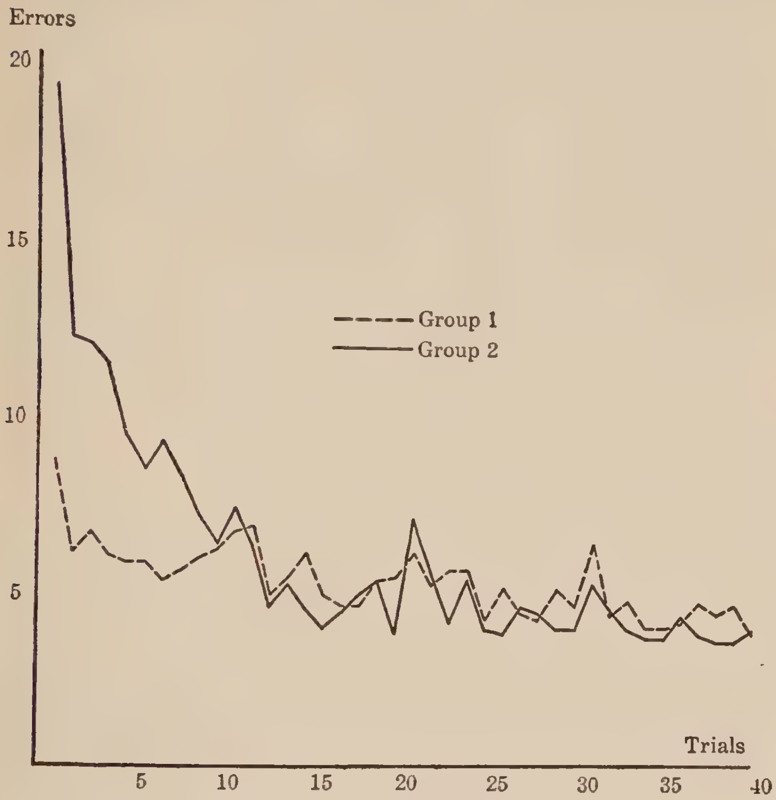

FIGURE 14 (Original FIG. 1)

Group I, the transfer group, had received a large amount of practice on the hand. In order to ascertain the effect of decreasing the amount of hand-practice, two new groups were obtained. Group III (11 Ss) was given 20 trials practice with the right hand, followed by practice with the foot. Group IV (12 Ss) 10 trials practice with the hand, followed by the foot. Comparing the averages of these two groups on the foot with that of Group I shows the effect of 60, 20, and 10 trials practice with the hand on the subsequent practice with the foot. The averages for the three groups are shown in Figure 15.

There are practically no differences between the three groups. With the exception of a few small variations the three curves are the same throughout.[32] Varying the amount of hand-practice from ten to sixty trials has no effect on transfer of learning to the foot. This means that the slight improvement characteristic of the hand-practice after the first ten trials is not transferred, or at least, that it has no observable effect in this experiment.

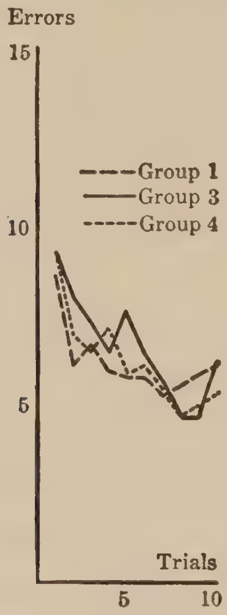

FIGURE 15 (Original FIG. 2)

A study of the individual curves of Groups III and IV reveals an interesting and important fact. In these groups the majority of the Ss show very little if any improvement on the foot after the first two or three trials. They show a small initial error and very little, if any, improvement. That is, they have almost nothing to learn. A few Ss, however, (a total of five in the two groups) show a large initial error and improve relatively slowly. Their curves are quite like the curve for Group II, the control group. Examination of the hand-records of these individuals shows that in every case either no improvement had been made on the hand or that the stage of slow improvement had not been reached when the hand-practice was discontinued. In all other Ss of these two groups the first drop in the learning curve of the hand had been completed and the secondary stage of slow improvement had been started when hand-practice was discontinued. For these latter Ss, curves for the foot typically like that for the average of the transfer group were obtained. It would seem that unless those adjustments which cause the first drop in the learning curve of the hand have been completed, no transfer takes place. Furthermore, these results show that the slow, gradual improvement, characteristic of the later periods of hand learning, has no observable effect on the foot.

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[32] This statement holds true only after one subject of Group III has been thrown out on the second and third trials. On these two trials this S was extremely poor, but he returned to about average in a few trials. On the second and third, he seemed to suffer a breakdown, lost all control over himself and appeared very embarrassed and self-conscious. Inclusion of his record on these two trials would distort the results of the rest of the group.

Transfer from Foot to Hand

In order to study transfer from foot to hand it was only necessary to give Group II practice with the right hand following its practice with the foot. Group II, therefore, was given 40 trials with the right hand following practice with the right foot. Comparison of its record on the hand with that of Group I gives the effect of previous practice with the foot upon the performance of the hand.

The averages of Group I and II on the right hand are shown in Figure 16. There is a slight difference in favor of Group II, which is, in this case, the transfer group. Although the difference is not great it is quite constant and lasts throughout the practice. The differences are not significantly greater than their probable errors. Consequently the question is raised whether the difference between the two groups is the result of transferred learning or whether it is not the result of a chance difference in ability between the two groups. In the work on transfer from hand to foot, transfer was only effective in the first part of learning. Likewise experiments on transfer from one situation to another have shown that transfer is effective only in the first stages of learning. When a rat, *e.g.*, has learned to run one maze, the training affects the first stages but not the later stages of learning a subsequent maze.[33] These facts would indicate that the results of the present experiment are due to chance rather than to transfer.

On the other hand, there is a possibility that transfer from foot to hand occurs but is obscured by a certain factor. During the hand-practice it was observed that the hand frequently changed position with respect to the arm while the subjects were hitting. In the great majority of subjects the change bore no relation to corrections but appeared to be a chance affair. The phenomenon was occasionally reported by subjects as a source of error that they found impossible to control. The hand could not be kept in a constant position with relation to the arm. This introduced chance errors into the experiment, and it was thought that a chance error of this sort might serve to mask transfer

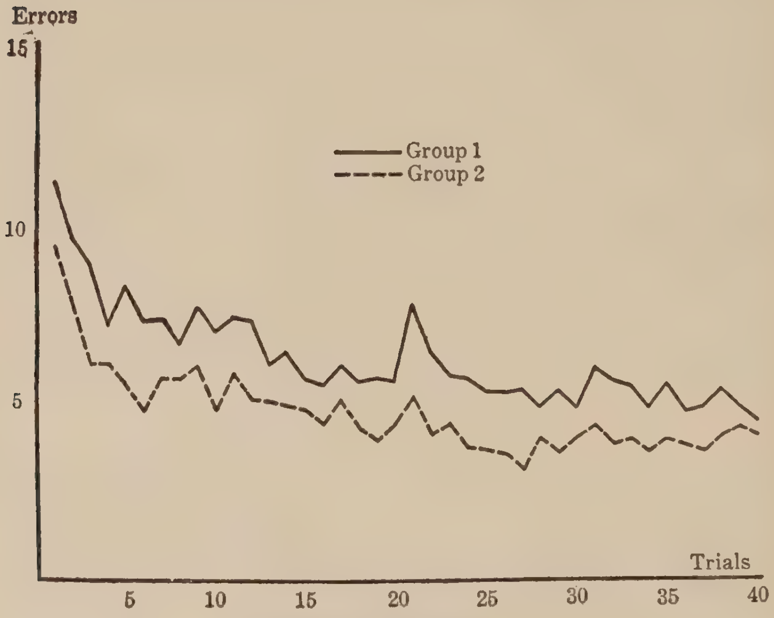

FIGURE 16 (Original FIG. 3)

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[33] L. W. Webb, Transfer of training and retroaction, *Psychol. Monog.*, 1917, 24 (no. 104).

effects. Accordingly a new experiment was devised in an attempt to rule out this factor.

The pencil was attached to a light piece of wood ten inches in length. On one end of the device a rounded block of wood was placed, which could be grasped comfortably by the hand. When held in the hand, the strip of wood extended nearly to the elbow, being strapped to the arm in two places. By means of this device, the wrist and hand were prevented from moving except in connection with movements of the whole arm, and the above mentioned source of error was eliminated.

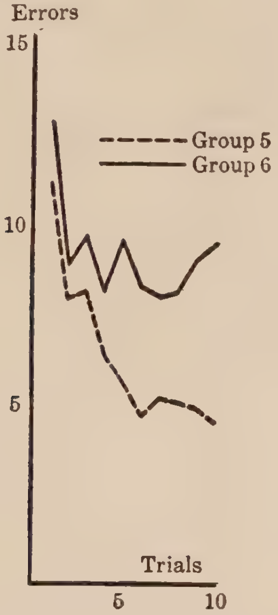

Figure 17 (Original Fig. 4)

Two new groups of subjects were obtained. Group V (10 Ss) received ten trials with the right foot followed by ten trials with the right hand. Group VI (11 Ss) received ten trials with the right hand. The averages of the two groups are compared in Figure 17. Throughout the practice there is again a slight difference in favor of the transfer group. On the last three trials the difference between the two groups increases due to the fact that three out of the eleven Ss in Group VI showed a marked increase in errors on these trials. In the case of two of these Ss it is believed that the increase in error is due to a loss of interest. Reports obtained from them after the experiment confirm this hypothesis. The result of this further study of transfer from foot to hand supports the conclusion that practice with the foot has some slight effect on subsequent practice with the hand, although the effect seems to be less than in the case of transfer from hand to foot.

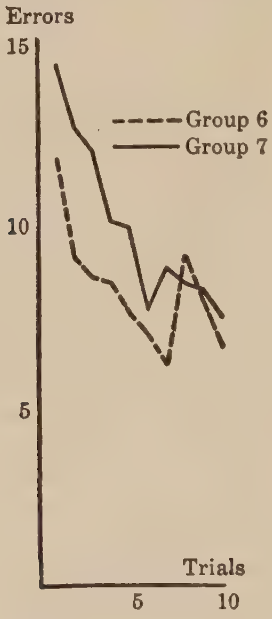

FIGURE 18 (Original FIG. 5)

Transfer from Hand to Hand

In order to study transfer from one hand to the other, Group VI, which had already received ten trials practice with the right hand, using the device for controlling hand and wrist movements, was given ten trials with the left hand, using this same device. Another group (VII) was given ten trials with the left hand followed by ten with the right. With these two groups transfer from right to left hand and from left to right could be studied.

In this experiment a new condition was introduced. When the subjects changed from one hand to the other the position of the mirror was changed. When the right hand was used, the mirror was in its normal position at the subject's left, as in the previous experiments. In using the left hand, however, the mirror was placed in a similar position but to the right of the subject. The change probably has little or no effect on the experiment, for it merely causes a shift in absolute cues, such as nearness to, and distance from, the mirror of objects near the target. On the other hand, the posture of the subject is considerably changed, for he must hold his neck in opposite positions in the two cases. It is not believed that this change affected the results.

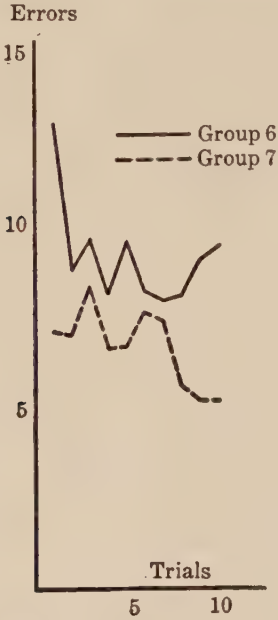

FIGURE 19 (Original FIG. 6)

In Figure 18 the averages of the two new groups for the left hand are shown. The graph shows that there is a slightly greater inaccuracy for the left hand as contrasted with the results already obtained for the right. But again the general fact of transfer stands out. For Group VI, the transfer group is slightly better than VII, the controls.

Quite similar results are found in Figure 19 where the curves of the two groups for the right hand are shown. These curves are not so smooth and regular as those previously shown; but Group VII, which in this case is the transfer group, presents a lower curve than VI.

In all the experiments reported here on transfer from one hand to the other, the differences between control and transfer subjects are small, and there is considerable overlapping between the control and transfer groups. Nevertheless, the fact that every group which has received previous practice with some other limb makes less error than its corresponding control-group is strong evidence for the existence of transfer.

The results, shown graphically above, are summarized numerically in Table I. The average of each group, together with its mean variation, is shown for the first ten trials with each limb. In a few cases the average of several groups taken as a single unit is also given. Thus Groups II and V, both of

TABLE I

RIGHT FOOT

	Control Subjects					*Transfer Subjects*						
	Group 2		*Group 5*			*Group 1*		*Group 3*		*Group 4*		
Trial	*Av.*	*M.v.*	*Av.*	*M.v.*	*Av.*	*Av.*	*M.v.*	*Av.*	*M.v.*	*Av.*	*M.v.*	*Av.*
1	19.1	5.9	20.5	8.8	19.7	8.6	2.9	9.2	4.3	9.2	6.0	9.0
2	12.0	3.4	13.9	7.4	12.8	6.0	2.6	7.9	3.0	6.9	3.6	7.8
3	11.8	5.4	10.9	3.7	11.4	6.6	2.2	7.3	1.5	6.4	3.2	8.0
4	11.3	3.3	8.1	3.2	9.9	5.9	2.1	6.4	2.7	7.1	3.4	6.5
5	9.2	5.6	8.3	2.9	8.8	5.7	2.5	7.6	3.7	5.8	2.9	6.3
6	8.3	4.3	7.0	2.9	7.8	5.7	2.4	6.4	3.3	6.1	2.4	5.9
7	9.1	4.4	5.7	1.5	7.7	5.2	2.3	5.6	2.2	5.4	2.3	5.4
8	8.2	2.8	6.3	2.1	7.4	5.5	2.4	4.6	1.4	4.7	2.0	5.0
9	7.0	2.2	5.6	1.7	6.4	5.8	2.1	4.6	1.8	4.9	1.8	5.1
10	6.2	2.0	5.6	2.1	5.9	6.1	3.0	6.2	2.2	5.3	1.6	5.5

RIGHT HAND

	Control Subjects							*Transfer*		*Control*		*Transfer*			
	Group 1		*Group 3*		*Group 4*			*Group 2*		*Group 6*		*Group 5*		*Group 7*	
Trial	*Av.*	*M.v.*	*Av.*	*M.v.*	*Av.*	*M.v.*	*Av.*	*Av.*	*M.v.*	*Av.*	*M.v.*	*Av.*	*M.v.*	*Av.*	*M.v.*
1 ...	11.1	2.5	11.4	4.3	13.7	5.6	12.1	9.3	3.0	12.7	3.9	11.0	4.4	7.0	2.3
2 ...	9.5	2.5	9.6	3.7	9.9	4.4	9.7	7.7	2.4	8.7	3.9	7.8	3.2	6.9	2.2
3 ...	8.8	4.2	8.9	4.2	9.0	4.4	8.9	6.0	1.4	9.5	3.1	8.0	2.9	8.2	3.8
4 ...	7.1	2.0	7.8	2.6	8.8	3.8	7.9	6.0	1.9	8.0	3.3	6.2	1.8	6.5	2.4
5 ...	8.2	1.8	7.5	2.9	7.6	3.1	7.8	5.5	1.3	9.4	4.1	5.5	1.3	6.6	3.0
6 ...	7.2	2.2	8.3	4.4	7.0	2.1	7.5	4.7	1.4	8.1	4.0	4.6	1.4	7.5	5.1
7 ...	7.3	2.4	6.8	3.2	8.6	3.3	7.6	5.6	2.2	7.8	2.6	5.1	1.0	7.3	2.8
8 ...	6.6	2.5	5.1	1.5	6.1	2.6	5.9	5.6	2.2	8.0	4.3	5.0	1.8	5.5	2.7
9 ...	7.6	2.2	5.7	1.9	5.7	1.9	6.3	5.9	2.8	8.8	4.7	4.8	1.6	5.1	2.5
10 ...	6.9	2.9	5.6	1.8	7.1	2.4	6.5	4.7	1.8	9.3	5.4	4.4	1.1	5.1	1.7

LEFT HAND

	Control Subjects Group 7		*Transfer Subjects Group 6*	
Trial	*Av.*	*M.v.*	*Av.*	*M.v.*
1	14.3	4.2	11.7	3.4
2	12.6	4.8	9.0	2.9
3	11.9	4.5	8.4	2.4
4	10.0	2.3	8.3	2.0
5	9.8	2.8	7.5	2.4
6	7.6	1.9	6.9	1.3
7	8.7	2.9	6.1	1.2
8	8.3	3.0	9.0	2.8
9	8.1	3.0	7.7	2.3
10	7.3	1.7	6.5	2.6

which commenced with practice of the right foot, are combined into one average. Likewise groups I, III and IV are combined for the right hand and foot. An examination of this table shows, as did the graphs, that in every case studied some transfer has been found. In order to facilitate comparison of the extent of transfer with practice by the various limbs, the percentage of error saved by each transfer-group in comparison with its corresponding control-group was calculated for the first ten trials. This calculation brings out merely the facts already noted that transfer is more effective from right hand to right foot than in any other case. The saving of groups transferred to the foot is reduced, however, to zero in the course of the first ten trials; whereas in the other cases the saving appears to last for an appreciably longer time, and in the one case where it has been studied it lasts for forty trials. No other differences in extent of transfer can be obtained, as the averages are probably not sufficiently stable to admit accurate comparison of the various groups.

DISCUSSION OF RESULTS

To explain transfer it is necessary to analyze the behavior of the subjects and to note the elements of practice that were common under the various conditions.

There were several factors which seemed to have no bearing on transfer and which may be readily disposed of. Thus the necessity for timing the hits in accord with the metronome and the necessity for returning to the starting position after every hit affected the accuracy of hitting. At the beginning of practice many subjects found it impossible to maintain the proper rhythm unless they attended closely to the speed of hitting. A few subjects likewise reported that the necessity to return to the starting position after every hit distracted their attention from accuracy and the reversing effect of the mirror. It has been shown by Schwarz[34] that in substituting a new habit for an old, a loss of attention causes the subject to revert to the old. In the present case the reversion would be to the normal habits of space co-ordination and would result in inaccuracy of hitting. It is not believed, however, that this factor was of importance in transfer, for no evidence of improved rhythm or better adjustment to amplitude of swing was found in transferred subjects.

One other factor of importance in learning but of little or no importance in transfer was the position of the body with respect to the arm or leg. The bodily position was not controlled in the experiments, the subjects taking a position comfortable to them. Usually the position remained constant throughout any one sitting but sometimes changes occurred. The effect of change seemed to be temporarily to increase error unless the change brought the upper arm and shoulder into line with the target. In most cases shifting the body increased error and thus was probably one factor in causing variability. This factor had no apparent bearing on transfer, however.

Of more importance to the study of transfer were certain other factors. Chief among these was adjustment to the reversing effect of the mirror. At the beginning of practice the first hit was largely a matter of chance. The second, however, almost invariably carried the subject farther away from the target because of the mirror-effect. The third hit might carry the subject still further away. Finally, a movement in the proper direction would be made. This was usually accomplished with a great deal of effort and in the hand-practice was frequently accompanied by movements of the whole body. A movement in the right direction was not, of course, always successful in bringing the subject close to the target. Consequently a further correction was needed which might result in still another error of direction. Errors of direction dropped out rapidly in the course of the first few trials and occurred but rarely at the end of the first sitting.

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[34] Untersuchungen zur Handlungs- und Affektspsychologie; iv. Ueber Rückfälligkeit bei Umgewöhnung; i Tl. Rückfalltendenz und Verwecklungsgefahr, Georg Schwarz, *Psychol. Forsch.*, 1927, 9, 105.

Next to the difficulty in adjusting to the reversing effect of the mirror, the greatest difficulty was to correct by the proper amount. There was an almost universal tendency to overcorrect; *i.e.,* in correcting a previous error there was a tendency to go beyond the target to the other side. This source of inaccuracy was also eliminated almost completely in the course of the first sitting.

It is believed that these two factors are the most important in the experiment. An effort was made to observe whether errors of direction and overcorrections occurred more frequently in control than in transfer groups. To this end the experimenter counted the number of times that either appeared to occur in any trial. The accuracy of counting in this way is undoubtedly low; but it is significant that all transfer groups showed fewer errors of direction and fewer overcorrections than their corresponding control groups.

The elimination of errors of direction and of correction was due in part at least to the development of methods. At the beginning of practice a few subjects tried to use methods, but these were usually found ineffective. In the course of practice, methods were developed, however, and were used to eliminate the two sources of inaccuracy mentioned.

In adjusting to the reversing effect of the mirror several methods were reported. The majority of the subjects reporting methods found it best to correct an error kinæsthetically. That is, when a previous hit had been to one side of the target the subject tried to reverse the direction of his movement and go in the opposite direction on the next hit, the directions being determined kinæsthetically. Visual perception of errors played little part in this process, being used only to determine the existence and amount of errors but not their direction from the target.

A second method, used by a few subjects, was to try to go further in the apparent direction of an error just previously made. Because of the reversing effect of the mirror this brought the subject back toward the target.

One further method was reported by two of our subjects, who observed that the side of the sheet of paper appearing further from the mirror was actually the side further away. That is, a hit at one side of the target might appear further from the mirror than the target itself and moving toward the mirror would correct in the proper direction. As this method was used by two subjects only, it was not very important in the experiment.

In order to overcome the tendency to overcorrect, a great many subjects reported that they made every corrective movement smaller than seemed necessary. Related to this method was the tendency (reported by nearly all subjects) to avoid trying to correct small errors. When a small error had been made, subjects found it better to refrain from any corrective movement and simply tried to repeat the previous movement.

One further factor that seemed of importance both in learning and in transfer was the initial nervousness and self-consciousness of some subjects. In many cases this was quite marked and was easily apparent to the experimenter. It is thought that this tended to decrease accuracy. After a few trials the nervous attitude disappeared and was replaced by an air of confidence. Usually, but not always, the confident attitude was carried over to practice with any other limb.

It was thought that an explanation of transfer might lie in the transfer of methods and in the elimination of the nervous attitude characteristic of so many subjects. Mere familiarity with the setting of the experiment might also be a factor in causing transfer. In order to test these various factors a new experiment was attempted in which the subjects were familiarized with the task and with the reversing effect of the mirror, and were given instructions as to the best methods to pursue in correcting errors.

On the spot usually occupied by the target a pendulum was set up. The screen was removed so that the pendulum could be seen both directly and indirectly. The subjects were requested to watch the pendulum both directly and indirectly as it swung back and forth, alternating from one to the other method of watching. It was pointed out to them that the mirror reversed the direction of swing, causing the pendulum and its image to go in opposite directions, and that it also made the right side seem at the left and *vice versa*. After several minutes spent in watching the pendulum the subjects were shown their task by watching the experimenter go through the usual procedure of a trial with the right foot.

Following this demonstration the kinæsthetic method of correction was carefully explained to the subjects, who were requested to correct all errors by this method. They were also told to try to make smaller movements of correction than seemed necessary.

A third instruction was also given the subjects. Observation of the behavior of previous subjects had led to the conclusion that some of the errors were due to watching the foot as it approached the line. Subjects who did so would try to make corrective movements of the foot as they were hitting. This seemed to result in further error because the rhythm of hitting was lost, because it led to a jerky, uneven swing that was not conducive to accuracy and because errors of direction were frequently made. No subject reported this as a factor of importance, however, in the experiment. Consequently it may have been a mistake to instruct the new subjects to try not to correct on the forward swing.

The three points mentioned were explained carefully to the subjects and the experimenter made as certain as possible that the subject understood them. Ten trials of the foot were then given to each subject of the new group. There were ten men in this group. One of the ten consistently failed to follow instructions as to correcting errors of direction and stated that he found it impossible to do so. His results were extremely poor and he showed no improvement throughout the ten trials. The average of the other nine is compared in Figure 20 with the average of all control subjects on the foot (Groups II and V) and with the average of all transfer subjects (Groups I, III and IV). It is shown that the instructed group is considerably better than the control group throughout the first ten trials. On the other hand, the new group does not show as great accuracy as the transfer group for a few trials. The effect of the instructions and familiarity with the experiment has been to decrease considerably the early errors but not to the extent that they are decreased by previous practice with the right hand.

There are several possible explanations of the differences between the instructed and the transfer groups. It may be that other factors than those on which instruction was given are present in transfer. Thus it was noted above that directional correction was often accompanied by movements of the whole body in the direction of correction. It is obvious that movements of this sort might play a large rôle in transfer. Similarly there might be movements of the eyes or head which would aid the subject in learning with the foot.

It may also be, however, that the difference between instructed and transferred groups is due to a lack of ability to follow instructions given in a merely verbal way. Nearly every subject in the instructed group reported that occasionally it was impossible for him to follow the instructions. Methods which had been learned by actual practice of the individual himself would

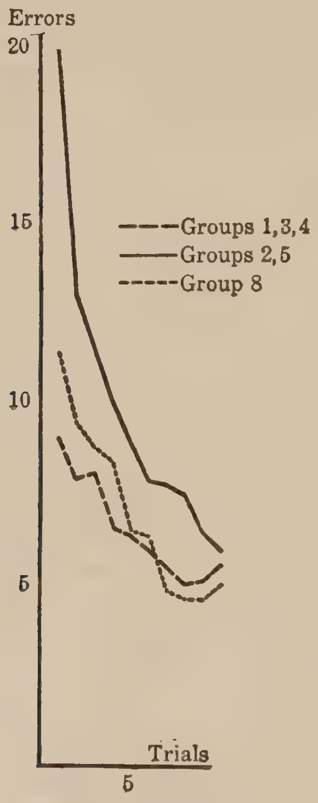

FIGURE 20 (Original FIG. 7)

probably be more successful than those given to him in verbal form.

It was also thought that the third instruction might have influenced the results. The instructed group may have received an advantage from this that the transfer groups did not have. Consequently a new group of subjects was obtained and given the same instructions and demonstration that had been given

Group VIII, except that the instruction with respect to correcting while on the forward stroke was omitted.

The results of this further work are shown in Figure 21, where Group IX (the new group) is compared with the control and transfer subjects on the foot. This curve represents the results of seven out of the nine subjects in the new group. Two subjects found it quite impossible to follow instructions and were omitted from consideration. The average of the other seven again falls between those of the control and transfer groups. It shows a slightly greater error than that previously given for Group VIII, the other instructed group.

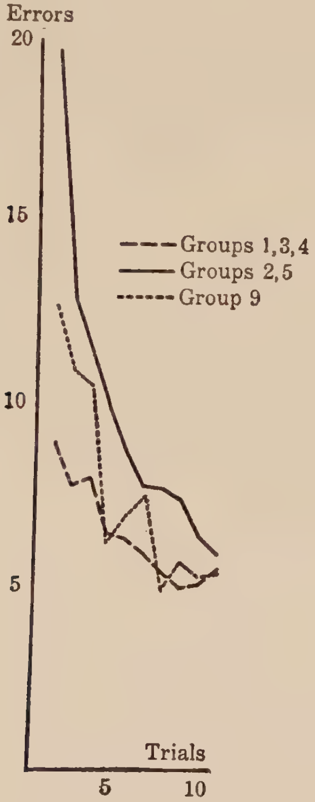

FIGURE 21 (Original FIG. 8)

The results of this work with the instructions support the hypothesis that carrying over previously learned methods to practice with a new limb accounts for a great deal of transferred learning. This conclusion is likewise supported by an observation on one subject of Group I who adopted a new method when changed from hand- to foot-practice. The curve for this subject was quite unlike that for the average of the transfer group and resembled closely that of the control group.

On the other hand, as stated above, it is impossible to say that transfer of method is the only factor of importance in transfer. The instructed groups do not show as great accuracy as the transfer groups, and it is not known to what this difference is due.

Summary and Conclusions

It has been shown that in the mirror-accuracy experiment a transfer of learning from right hand to right foot takes place. The effect of previous practice with the hand is to increase considerably the initial accuracy of the foot and to save the equivalent of about six trials. It has also been shown that varying the amount of hand-practice from ten to sixty trials has no observable effect on transfer to the foot. There are also indications that transfer does not occur if the first fall of the learning curve of the hand has not been completed.

Transfer from right foot to right hand also occurs. It seems to be less in amount but effective over a longer period than transfer from hand to foot. Likewise a slight transfer effect is observable from one hand to the other.

The explanation of this form of transfer probably lies, in part at least, in the transfer of methods which having been learned for one part of the body are carried over to practice with some other part. Mere familiarity with the setting of the experiment is probably also important in transfer.

GLOSSARY

Bilaterally Symmetrical, parts duplicated in form and position on both sides (of the body).

Faculty, ability, capacity; complex mental process erroneously regarded as an unanalyzable process.

Hemiplegia, paralysis of one side of the body generally resulting from injury to the opposite side of the nervous system.

KINÆSTHETIC, muscular; pertaining to the "sense" of muscular movement and strain.

MIRROR WRITER, person who executes writing that is reversed right and left; this writing can be read in a mirror.

MOTOR CENTERS, areas of the cortex of the brain having to do with muscular co-ordination.

NEUROMUSCULAR, pertaining to the nervous system and the muscles.

S, subject; observer.

GROUP V

REACTION AND OBSERVATIONAL BEHAVIOR

READING EIGHTEEN[1]

THE EFFECT OF UNIFORM AND NON-UNIFORM ILLUMINATION UPON ATTENTION AND REACTION TIMES, WITH ESPECIAL REFERENCE TO STREET ILLUMINATION[2]

Editor's Note

The next convenient step in the restriction of conditions under which human behavior takes place causes two limited but important aspects to emerge. The one is simple reaction-behavior and the other is observational behavior. Remember that, as a consequence of this gradual restriction, more and more simple modes of response have been abstracted from the social behavior studied at the outset. But the whole was deliberately ignored when the process of analysis began. This process was commenced in an investigation of intelligent behavior. Notice that the original, social behavior was intelligent and that the process of analysis resulted in an emphasis upon *a mode of behavior already encountered,* for it was an aspect of the whole. A "smaller" whole had been segregated from a larger one for the purpose of specialized study.

The next step in the restriction process resulted in a segregation of emotive behavior from the same whole. Emotion, too, had been encountered in studying the whole. Likewise with learning, for the learning process in human beings goes on normally only in a social environment *as an aspect of a behavior whose primary characteristic is social.* But in isolating typical examples of learning for the purpose of analysis, the whole of which it is normally a part receded still farther into the background. This does not mean that learning never takes place where social relations stand out more prominently, as in the interplay between personalities. Indeed, the development of personality is a learning process. This shows how erroneous it is to suppose that different kinds of behavior are parts, in the sense of *elements,* like the tiles of a floor, each having independent existence. They are parts only in the sense of aspects. Thus it is necessary to remember that social behavior is intelligent and intelligent behavior is social; intelligent behavior is emotive and emotive behavior is intelligent; learning is social, intelligent and emotive. In every form of behavior no one aspect is separable from the others except as a particular aspect is segregated, artificially, by the method of restricting its conditions.

Reaction- and observational behavior are not exceptions to this rule.

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[1] Parallels *The Science of Psychology,* Chapter XII.

[2] By Harold E. Burtt, Ohio State University. From the *Journal of Experimental Psychology,* Vol. 1, 1916, 155-182. By permission.

They have been encountered before, continuously, in fact, but they were not always labelled at the time. They are now to be studied as segregated wholes.

Simple reaction-behavior is the process of executing a relatively simple and brief co-ordinated movement in response to a simple stimulus-situation. Everyday life abounds with examples. An alarm-clock going off in the morning is the stimulus for the reaction of jumping out of bed. A dog running out in front of an automobile is the stimulus-situation for the reaction of throwing out the clutch and putting on the brakes. A pheasant flying up from the grass is the stimulus-situation for the reaction of aiming the gun and pulling the trigger. In a track-meet, a pistol-shot is the stimulus for the reaction of getting under way as rapidly as possible in the hundred-yard dash. In boxing, a certain motion of the opponent's fist is a stimulus for the reaction of dodging.

In the psychological laboratory this type of behavior can be brought under control and the conditions for its quickness and accuracy can be studied in detail. This requires special apparatus of different kinds, especially timing devices, known as *chronoscopes*. (Chronoscope comes from two Greek words meaning time and measure.)

Reaction-behavior introduces the student to observational behavior. One must observe the stimulus-situation, particularly certain aspects of it, in order to react. Observational behavior has been encountered, heretofore, under the name of perception. It should be obvious that all forms of behavior involve it.

The term observational behavior is best limited to the process of noticing, or perceiving objects as wholes in their relation to total situations. Simple examples were indicated in connection with reaction-behavior. The awakened sleeper *hears* the alarm clock; the driver *sees* the dog; the hunter *sees* the pheasant. Observational behavior, then, brings one directly into contact with the functioning of the organs of sense, which are important conditions of all behavior.

The organs of sense are located in the eye, ear, nose, tongue, skin and muscles, and will be the subject-matter of detailed study later on. They are not, however, the exclusive conditions of observational behavior. There is a set of conditions pertaining to the total behavior of the individual at the time of observation, that are extremely important in determining what shall be, and what shall not be observed. Among them are purpose in mind or "mental set," the emotive aspects of behavior at the time, the intensity of the stimulus, and change of stimulus.

The conditions of observation, just named, are also known as the conditions of attention. Attention is popularly regarded as a special kind of mental process, the process of *selecting* for observation at a given time certain objects or situations *to the exclusion of others*. By implication, therefore (unless one is very careful), attention becomes a special kind of mental process, a selective agent, when in reality it is nothing more than the process of observing under particular sets of conditions. The conditions, not attention, control the perception. Attention is a term indicating that the perception is controlled. For a long time in the history of psychology, attention was considered an explanatory concept just as association was, but, like association, the usefulness of the concept in an organismic system can be doubted.

Professor Burtt's use of the term attention can be interpreted to mean the conditions of observation. His investigation is concerned with the conditions of reaction- and observational behavior, and illustrates experimental methods in this field. His results show how total stimulus-situations induce tensions in the individual, which, under the circumstances of his experiment, were resolved in the course of fulfilling instructions. It is interesting to note that the increased tension induced by non-uniform illumination exhibited itself in greater alertness of observation, faster reactions, and greater efficiency of motor co-ordination. Other things being equal, the greater the tension to be released, the more immediate the goal and the better organized the configurational process of reaching it. This fact was also evident in learning when learning involved emotional tension in the form of interest and motives. Professor Burtt's results need not be taken as a recommendation for poorly lighted streets, but as an excellent illustration of functional analysis.

A. INTRODUCTION

The problems of street illumination have not been approached hitherto from a psychological standpoint. The principal considerations have been photometric and æsthetic, but it is evident that these are insufficient. Mere ability to discern surface irregularities, vehicles, pedestrians or obstacles on the street or sidewalk does not insure safety unless the reaction to these stimuli is normal. If, under a given system of illumination, the reactions of the driver or pedestrian are retarded and the degree of attention diminished, the danger is manifest. From casual observation it would appear that some such factor is present. Many automobilists speak of feeling ''sleepier'' under certain varieties of street-lighting. Moreover on theoretical grounds the monotony of some lighting systems might be expected to influence the mental state.

During the summer of 1914 the writer was connected with an experimental study of street illumination conducted by the Street Lighting Committee of the National Electric Light Association on Intervale Avenue, New York City. It was found feasible to test the attention and reaction times of a subject walking on the street under various conditions of illumination. The results thus obtained proved rather significant and the following winter the problem was reduced to laboratory form and experiments conducted in the Harvard Psychological Laboratory. The following article thus comprises two parts, an account of the experiments performed upon the street and of the supplementary experiments in the laboratory.[3]

B. EXPERIMENTS ON THE STREET

I. *Equipment*

The following experiments were conducted at Intervale Avenue, Bronx, N. Y. Two sections of the avenue, each a third of a mile in length were equipped for experimental purposes. In one section series circuits fed lamps mounted at the curb 14 ft. above the street, staggered at 50 ft. intervals, with an alternative system of centrally hung lamps 24 ft. above the street at 150 ft. intervals. In the other section the lamps were suspended from span wires and could be moved to any position transverse the street, or from a height of 18 to 30 ft. Provisions were made for mounting series lamps at 75 ft. intervals with two series circuits feeding alternate lamps. The circuits were operated from tub transformers in a temporary sub-station supplied by a 2,200 volt circuit from the Hunt's Point sub-station of the New York Edison Company. The street was 60 ft. wide from curb to curb and paved with Belgium block. Dwelling houses, small apartments and a few stores lined the sides. The street was reasonably free of traffic during the evening.

II. *Method*

Certain members of the Street Lighting Committee conducted experiments designed to bring out the ability to discern surface irregularities and obstacles in the street under various conditions of illumination. The writer confined himself to attention, reaction times and motor co-ordination. The efficiency of the eye, while undoubtedly of importance where a person is exposed to street illumination for a prolonged period, as in the case of the motorman, was not studied. It was deemed advisable to restrict the experiments to relatively short periods of continuous exposure to a given illuminating system—the condition encountered in the case of the average pedestrian.

To measure the quickness of response to unexpected stimuli and the ability to make a quick association and carry out the appropriate movement, the auditory choice reaction was adopted. Auditory rather than visual stimuli were used because they left the subject free to look down the street as he would normally. It was desired to reproduce as closely as possible the situation that is met in practical conditions, and with red and green lamps for stimuli the subject would tend to keep his eyes fixed on the point from which the stimulus was to come rather than directed down the street. Two electric bells of distinctly different pitch were connected in parallel. In series with them were two presselles at the end of 10 ft. of lamp cord, a signal magnet and dry cells. The magnet and a 100vd. electric tuning fork recorded on a kymograph. The whole apparatus was mounted rigidly and clamped to a table built on the truck of a baby carriage. The subject walked down the traffic line midway between the center of the street and the curb, holding a presselle in each hand. The experimenter followed at a distance of about 8 ft. wheeling the apparatus. The subject was instructed to look down the street as he would under ordinary conditions, and when he heard the "cow-bell" to release the right hand and when he heard the "door-bell" to release the left. Trials for the right and left hands were given in irregular orders and at irregular intervals without warning. Right and left reactions registered similarly and were averaged together, approximately the same number of both being given in a series. Incorrect reactions were few (not more than 5 per cent.). The subject reported mistakes and such reactions were omitted from the results. The subject walked continually except when it was necessary to stop a moment to shift the drum of the kymograph. The subject always passed through the complete distance between lamps or multiples thereof, depending on the spacing, so that reactions were made at approximately all degrees of illumination under a given system. Fifteen to twenty reactions constituted a series according to the capacity of the drum, *i.e.*, from 60 to 80 reactions for a subject on a given comparison.

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[3] The writer expresses his obligations to Professors Hugo Münsterberg and Herbert S. Langfeld and to Messrs. Preston S. Miller and W. F. Little, of the Electrical Testing Laboratories, New York City.

A form of short exposure test was adopted as a measure of attention. A box was constructed 30 × 30 × 30 cm. with a 15 cm. tapering extension on the top through which the subject could look at a field 45 cm. from the eyes. The interior was illuminated by two battery lamps screened from the eyes of the subject and controlled by a switch and an opening at the bottom of one side of the box allowed cardboard material to be inserted. This tachistoscope was clamped on the baby carriage. Simple geometrical figures such as a square, circle, trapezoid, etc., constituted the material. It was desired to test not merely memory for a series of separate figures, but rather the ability to analyze them out of a complex group, which necessitated a high degree of attention. Accordingly five were drawn in outline with ink on light gray cardboard 30 × 30 cm. so that four of them touched at the center of the card, above, below, right and left, and the fifth was concentric with the field. The subject placed his eyes at the opening and one second after the signal "ready" the field was exposed for one second by closing and opening the switch in time with the swings of a half-second pendulum attached to the side of the tachistoscope. Immediately after the exposure the subject drew upon a pad as many as possible of the figures in the correct position.

Exposures were made at intervals of 15 ft. along the traffic line, a new permutation of objects being shown each trial. When walking between trials the subject kept his eyes directed down the street as he would normally. In scoring the results a figure in the correct position was counted 2, and a figure merely disoriented or displaced less than a quadrant from the correct position counted 1. The benumbing or quickening effect of a given illuminating system on the mental setting would presumably not disappear in the few seconds that elapsed while the subject was lowering his head to the opening of the tachistoscope and awaiting the exposure after the signal "ready." The gray cardboard was used as it afforded less contrast with the street surface upon which the subject had been gazing a moment before. Ten exposures usually constituted a series, in some cases twenty—*i.e.*, forty or eighty trials on a given comparison. In some instances several subjects (never more than five) were used at the same time, each one stepping aside to make his drawing while the next one observed the figures. Care was taken that no exposure should be made while there was any distracting motion among the subjects.

As a measure of motor co-ordination a "three-hole" test was employed. A board 20 × 20 × 1 cm. had three 12 mm. holes bored in it at the vertices of an equilateral triangle 10 cm. on a side. A brass plate covered the bottom of the board and was connected in series with a signal magnet and dry cells. A tapping stylus formed the other pole of the circuit and contacts made at the bottom of the holes were registered by the signal magnet on the kymograph. The subject was instructed to insert the stylus in each hole successively as rapidly as possible, being sure to touch the brass plate at the bottom of each hole before passing to the next. The results were thus reduced to terms of speed. The test was always performed under a lamp where the holes could be plainly seen, the inside of them being painted black to render them more visible against the white board. The actual illumination of the board was not of importance as long as the holes were clearly visible and there was no distracting reflection of light from the board. The subject tapped for 30 seconds from the signal "ready—go" until the signal "stop." Four or five trials were given after each choice reaction series. Between trials the subject looked down the street.

The main interest of the experiment was in the comparative results under any two systems of lighting. With each subject two equipments were used on the same evening, so that variables such as temperature, weather and physiological condition were fairly constant. The tests were always performed in both time orders—equipment *A* followed by equipment *B* and then after an interval of 10 or 15 minutes, *B* followed by *A*. The average of the two series on the first equipment was compared with that of the two series on the second. A short practice series was given each subject before proceeding to the street for the first series on a given method. The subject always walked for 5 minutes on a given street before being tested.

For all the above experiments only short sections of the two streets were used. This was because one street passed through a square with cross traffic and a number of brightly lighted stores. Two short sections were selected which were fairly similar in conditions along the side. In one case there was a small garage and in the other a delicatessen store on the side of the street used and these were frequently closed during the later hours. Precautions were taken against undue distraction in all the experiments. No trials were made when a vehicle of any sort was passing or when there was any unusual disturbance on the sidewalk. Conversation during a series was avoided. In only two or three cases were the subjects at all aware of the trend of previous results. All possibility of suggestion was avoided. The subjects were employés of the Electrical Testing Laboratories of New York City, and of the New York Edison Company, the majority of them between twenty and thirty years of age. The experiments were performed between 8 P.M. and midnight.

III. *General Results*

Experiments carried on by the above methods indicate that of the illuminating factors influencing the attention and mental reactions, uniformity and non-uniformity of illumination are paramount. The main thesis that seems established by the experiments on the street is that under a distinctly non-uniform illumination (with a mean variation in foot-candles of upwards of 50 per cent.), the attention of an individual walking through the street during a period of 20 to 30 minutes is at a higher level and his reaction time is quicker than when he is walking under a distinctly uniform illumination during a similar period.

Table I gives the results of all the experiments in which a markedly uniform system of lighting was compared with a markedly non-uniform. In the first column are given the two equipments compared on a given evening. Nitrogen gas filled Mazda lamps were always used. To produce uniform illumination, 80 candle power lamps in Holophane units with refractors were mounted 14 ft. above the curb, staggered at 50 ft. spacing, or 250 c.p. lamps in Holophane units with refractors were mounted 24 ft. above the center of the street at 150 ft. spacing. The non-uniform illumination was usually produced by 250 c.p. lamps in diffusing globes (the Edison AB unit) centrally hung 24 ft. above the street at 150 ft. spacing, or by 400 c.p. lamps in Edison units mounted 30 ft. above the street 7½ ft. out from the curb and staggered at 300 ft. intervals. In a few cases bare lamps were used.

The results show that in every case the auditory choice reaction time is shorter under the non-uniform equipment and on the average is 17 per cent. shorter. The differences are all greater than twice the probable error of difference and average nearly four times as great. The differences between the per cent. mean variation under uniform and non-uniform lighting is small. Attention, as indicated by a tachistoscopic test, is superior under non-uniform illumination in 26 out of the 31 series. The average superiority, including the negative values, is 8 per cent. The differences are on the average twice the probable error of difference. The mean variation is considerably greater, about 30 per cent. on the average, under the uniform condition. In motor co-ordination three-fourths of the series show greater efficiency under non-uniform lighting, with an average superiority, including negative values, of 3.6 per cent.

The differences are on the average nearly twice the probable error of difference. In most cases the mean variation is greater under the uniform conditions—48 per cent. on the average.

The above results are briefly indicated in Table I. The successive columns give the number of series performed under each of the three methods, the per cent. of that number in which the non-uniform series is superior, the average superiority of the non-uniform, the per cent. of series in which the greater mean variation (*i.e.,* lower degree of attention) is shown under the uniform lighting and the average of the quotients obtained by dividing the mean variation of the uniform by that of the non-uniform. The general tendency in favor of a quicker reaction and co-ordination and a higher degree of attention under the non-uniform equipments is evident.[4]

TABLE I

	No. of Series	*Per Cent. of Series in Which Non-uniform Superior*	*Average Superiority Non-uniform Per Cent.*	*Per Cent of Series in Which Uniform Had Greater M.v.*	*Av. M.V. Uniform / M.V. Non-uniform*
Choice reaction	18	100	17.0	50	.98
Tachistoscope	31	84	8.1	81	1.32
Motor co-ordination ..	22	77	3.6	82	1.48

IV. *Checks*

A number of other variables that might have been instrumental in producing the above results were ruled out by suitable check experiments.

As a check on the possible influence of the nature of the street in distracting elements tests were made on a few subjects with identical equipments on the two streets. The average differences were 6 per cent. and 2 per cent. on reactions and on the tachistoscope respectively. Moreover the uniform equipment was sometimes on the north street and sometimes on the south street with little difference in results. Further, the same street was occasionally employed for all four series on a given evening. Here the sidewalk, traffic and window lighting conditions were constant, the only variable being the illuminating conditions.

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[4] Simple auditory reactions were tried with two subjects and yielded results similar to those with choice reaction. These two types were correlated later in the laboratory work.

As to the type of illuminant used and the possible effect of Edison *vs.* Holophane units, some reaction series were made, using bare lamps in one or both streets. A street equipped with Edison units, compared with one identical but minus the globes yields slight differences in reaction. But if the Edison equipment is compared with bare lamps with refractors giving uniform illumination, or if two systems of bare lamps are used, one with refractors, the usual differences occur.

As to the intensity of illumination, the photometric data show that in approximately half of the comparisons the mean intensity of the non-uniform system was greater than that of the uniform and in the other half the reverse was true. The experimental results favored the non-uniform lighting under both conditions.

The time error was checked as above indicated by working on both equipments on a given evening in both time orders. Further, the uniform equipment was tested on the first series in about half of the comparisons.

As to the possible influence of number of lamps, cross street location, height or spacing, 250 c.p. lamps in Edison units were centrally hung 24 ft. high at 150 ft. spacing and compared with similar lamps in Holophane units with refractors having exactly the same mounting. In a few of these series moreover the two equipments were on the same street. In this rather crucial check it is evident that the usual tendency is manifest to about the usual degree.

Finally the question might be raised as to whether the superior results under non-uniform illumination are not due to a sort of voluntary alertness produced by the difficulty in seeing objects in the dark areas, that is, whether the results are not due to the dimness of the lighting rather than the non-uniformity. As a partial answer one subject was tested in reaction times on a uniformly lighted street run at the usual 7.5 amperes and then at 6.1 amperes. The reactions were 16 per cent. quicker under the former condition.

V. *Degrees of Non-uniformity*

The above results point rather strongly to the superiority of non-uniform illumination of the types studied in its effect upon attention and reactions of the pedestrian. Whether the degree of attention varies with the degree of non-uniformity is doubtful. In the present study there were two non-uniform equipments that were used for the most part, with 150 ft. and 300 ft. spacing, the latter of which was more non-uniform in both horizontal and vertical illumination. Both of these equipments were frequently compared with a uniform system which might be regarded as the standard. The 300 ft. spacing shows a greater average superiority over the uniform system in choice reaction than does the 150 ft. spacing, but in motor co-ordination and attention this condition is reversed. A few tests were made comparing directly the two non-uniform equipments on the same evening with the same subjects. The 300 ft. system was slightly superior by all three methods.

This problem of degrees of uniformity could be answered only by exhaustive investigation. Even if the degree of attention were found to be a positive function of the degree of non-uniformity, it is probable that beyond a certain point the dimly lighted stretches of street would be so broad that the effect would amount to uniformity and produce retardation.[5]

C. Experiments in the Laboratory

In transferring the problem to the laboratory there were five questions in mind:

1. How far do results obtained with the factor of uniformity and non-uniformity of illumination controlled in the laboratory substantiate in general the results obtained on the street?

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[5] The results of the experiments carried on at the same time by other investigators to determine the revealing qualities of the different equipments for surface irregularities and obstacles in the street, tend somewhat in the same direction as the above results, rather than in the opposite direction. Targets of wood 2 inches thick or of galvanized iron plate the size of the surface of a paving block, painted gray and sanded were planted systematically through the two streets. Subjects walked along the traffic line at their normal rate, recording on a pad all the targets they saw and their location. It was found in general that these targets were perceived somewhat more readily on the street with the large illuminants widely spaced than on that uniformly lighted by small illuminants closer together. Furthermore with the equipments studied the targets were found almost as successfully in the darker areas between lamps as in the lighter areas. In other tests sections of stove-pipe one foot high, painted gray and sanded, were planted at various positions and the subjects in an automobile rode through the streets at constant rate with a device which recorded the maximum distance at which each target was seen. Results thus obtained tend in general in the same direction as those with the surface targets. The above results are not as yet published and no conclusions should be drawn from them, but their general tendency is of interest in the present connection.

2. How far do other kinds of reaction correlate with auditory reactions under the various illuminating conditions?
3. What are the effects of different degrees of non-uniformity?
4. Can the results in favor of non-uniform illumination on the street be attributed to greater voluntary effort in watching for obstacles in the dark regions between lamps, *i.e.*, does voluntary attention to one stimulus quicken reaction to another?
5. Can the results be attributed predominantly to the changing illumination through which the subject walks or to the light and dark patches on the street surface?

I. *Equipment*

The experiments were performed in an interior room with black walls, indirectly illuminated by light reflected from the white ceiling. The source was about 1.5 meters from the center of one end of the room directly above the head of the subject, and consisted of a 100-watt tungsten lamp in a box 60 cm. square and 45 cm. deep suspended 60 cm. from the ceiling. The subject sat alone, the experimenter and all the apparatus being in an adjoining room in front of the subject.

With the pedestrian walking on a non-uniformly lighted street there are two possible factors which may influence his mental state—the varying illumination intensity through which he is passing and the alternating light and dark regions on the street surface before him. To reproduce the first of these a 110-point circular rheostat was placed in series with the lamp illuminating the room. An alternating-current motor, geared down by belts and speed reducers, was arranged to drive a crank. One end of the crank was connected by a shaft with the margin of a wheel of greater radius, so that the latter oscillated through 160 degrees. This was belt-geared to a disc attached to the axle of the rheostat. Thus, when the motor was in operation the illumination in the room where the subject sat was slowly dimmed to a definite point (not, however, to the point where the color of the light changed perceptibly), then increased and this process continued. These conditions roughly paralleled those to which the individual on the street is subjected as he walks along, the brightest point in the cycle in the laboratory corresponding to the position under a street lamp, the darkest part of the cycle to the position midway between lamps and the intervening points in the cycle to the intermediate positions on the street. The speed reducers were combined to produce complete cycles of 25, 55 or 85 seconds.

To reproduce the second factor, the shadows on the street surface, a black box with light interior 115 × 35 × 20 cm. with one of the 115 × 20 edges open was mounted vertically on a table at the subject's right with the open edge facing the left at an angle of 60 degrees to the wall. In strips attached to the edges of this side a piece of cardboard 115 × 20 cm. with 17 × 5 cm. holes in it 5 cm. apart could be moved up and down. In the center of the opposite side of the box was a 25-watt tungsten lamp. Thus a slightly fan-shaped array of shadows was cast on the wall directly in front of the subject. The cardboard was weighted at the bottom and attached at the top to a cord which ran through pulleys over the partition into the adjoining room. The cord could be attached to the crank which actuated the rheostat and the shadows thus be made to move up and down at various rates through a distance of 40 cm. This roughly reproduced the slowly changing effect of the light and dark patches on the street surface as the subject walks along under non-uniform lighting.

To parallel the uniform conditions on the street, the room was left at its normal condition with the lamp at maximum intensity. The average brightness on the black wall in front of the subject was then .119 meter candles.[6] When the light was dimmed to the lowest point of the cycle the average brightness was .037 m.c. With the shadows the average figure (averaging maxima and minima of the shadows and of the light spaces between them) was 1.38 m.c. When the shadow equipment was compared with a uniform, the card in the latter case was drawn up above the box and the light in the box shunted through the rheostat and held constant at a point which gave approximately the same average brightness on the wall. A point was found on the rheostat which gave a brightness of 1.42 m.c. as compared with the 1.38 m.c. for the shadow equipment run at the full current.

II. *Method*

Experiments were made under these conditions upon various sorts of reaction times and upon attention as involved in a tachistoscopic test similar to that employed in the street experiments. For the latter purpose a portion of the wall between the subject and the experimenter was replaced by a piece of black cardboard 66 × 100 cm. in the center of which was a piece of ground glass 10 cm. square. Behind this was a rack into which paper or cardboard material could be inserted. Arrangements of geometrical figures in combinations of five, quite similar to those described above (p. 342) were employed. The figures were 4 sq. cm. in area, drawn with a soft pencil on white linen paper. When placed in the rack pressing against the ground glass and illuminated by a lamp 20 cm. distant they were plainly visible to the subject on the other side of the glass. The exposures of one second were made by covering the end of one of the speed reducers with a wooden disc with a sector of proper magnitude removed and arranging a contact of spring brass to press on the disc. The spring contact and the speed reducer were connected to two poles of the lamp circuit which was thus closed during a part of the revolution of the reducer. A telegraph key in the circuit enabled the experimenter to make the exposures whenever he wished. After the exposure the subject reproduced the figures on a small square of paper on the table at his side and placed his drawing in a drawer which the experimenter pushed through the wall for the purpose and which was then withdrawn. In scoring, a figure in the correct position counted 2 and a figure displaced less than a quadrant or disoriented counted 1. A different permutation of objects was shown each trial during the first series on two lighting equipments in one time order, and then the same material repeated on the two equipments in the opposite time order. That is, each pattern was used once on each of the two equipments under comparison, thus eliminating any error due to degrees of difficulty in the material.

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[6] Photometric measurements were made over an area of approximately one square meter with a Lummer-Brodhun photometer, using an amyl-acetate lamp as a standard.

Auditory choice reactions with the feet were sometimes intermixed with the short-exposure tests and sometimes used alone. Keys were made of two pieces of wood 20 × 10 × 1 cm. hinged at one of the longer edges, with brass strips for contacts at the outer edges and held apart by light springs. Each foot rested on one of these keys, the normal weight of the foot being sufficient to close the circuit. The stimuli consisted of a small electric gong and a sounder made by removing the bell from a larger gong so that the armature struck the magnets. The time was measured with two electric vernier chronoscopes. By the use of a double-throw switch, the two chronoscopes could be operated through the same connections and thus could be used in quick succession and counted simultaneously. The counting was facilitated by giving the second stimulus on the 10th, 15th, etc., swing of the first. The chronoscopes were always callibrated at the beginning of the hour. For simple reactions a telegraph key on the table beside the subject was connected in parallel with the foot contacts.

For the stimulus for a simple visual reaction a battery lamp on a frame, so hinged that it could be swung near the ground glass before the eyes of the subject, was placed in parallel with one of the bells by means of a double-throw switch, and a piece of cardboard with a 6 mm. round hole placed between it and the glass. The subject thus received a small patch of light as a stimulus.

In conducting the experiment, two equipments were always employed on a given hour in both time orders, *i.e.*, uniform, non-uniform, non-uniform, uniform, or the reverse. The subject sat passively under the given equipment for approximately 3 minutes prior to each series. On a given comparison half the subjects began with the uniform and half with the non-uniform equipment, and when the same subject worked two different hours on a given comparison, the second hour began with the equipment the opposite of that of the first hour.

The instructions for the short-exposure test (after a preliminary explanation of the material and observation of a sample exposure) were, "Reproduce as many of the figures as possible in the correct position." Those for the choice reaction were "When you hear the bell lift the right foot and when you hear the sounder lift the left." When the reactions were intermixed with the short-exposures the instructions were a combination of these two: "When you hear the bell lift the right foot, when you hear the sounder lift the left, and when you see the pattern reproduce as many of the figures as possible in the correct position." For the simple reactions the subject was instructed: "Motor reaction to the bell" or, "Motor reaction to the light." The instructions were repeated before each of the four series during the hour. At the beginning of the hour three patterns and ten reactions of the sort to be used were given for warming up, and the first two reactions of the four series were omitted from the results.

The number of trials varied according to the combination of tests used on a given hour. The tachistoscope tests were always given in series of 40, *i.e.*, 20 on each equipment. When choice reactions were used in connection with the tachistoscope there were 80 trials, 40 with the short exposures intermixed and 40 without. With simple reactions it was possible to give 240 per hour, usually 120 auditory and 120 visual.

The reactions and exposures were given at irregular intervals. The order for the choice reactions was determined by shuffling cards containing the same number of right and left at the beginning of each series. When the reactions were intermixed with the tachistoscope, cards marked "T" were intermixed with the others before shuffling.

The subjects were graduate students in the Harvard Laboratory and a few undergraduates doing advanced work in psychology. None of them knew the trend of the results of the experiments on the street. Ten subjects participated at different times.

III. *General Results*

Various combinations of uniform and non-uniform illuminating conditions were studied comparatively and the results tend in general to substantiate those obtained on the street.

From the averages of measurements taken it is evident that there is a general tendency toward greater efficiency under non-uniform illumination. Simple auditory reaction time is superior under such lighting in 82 per cent. of the series, an average of 5.8 per cent. Simple visual reaction time is quicker in 65 per cent. of the series, an average of 3.2 per cent. Auditory choice reactions with tachistoscopic trials intermixed yield similar results in 65 per cent. of the series with an average superiority of 2.5 per cent. Auditory choice reactions without the tachistoscopic trials tend in the same direction in 65 per cent. of the series, an average of 2.8 per cent. And the tachistoscopic experiments tend likewise in favor of non-uniform illumination in 55 per cent. of the cases with an average superiority of the non-uniform of 1.9 per cent. The differences in general are between 1.5 and 2 times the probable error of difference and in the case of the visual reactions over 3 times. The mean variations do not show as marked a tendency, although they average in each method slightly in favor of the non-uniform condition. It might be noted that the individual averages in the simple reactions are based on a larger number of trials and the differences prove greater.

At the end of each hour the subjects were asked whether they had any introspection as to their comparative mental state under the two systems of illumination. In a large majority of the series (83 per cent.) the subject had nothing of interest to report. The usual statement was: "Felt no difference under the two systems"; or "Think I did as well under one system as the other." It is evident that an individual may be influenced by the illuminating conditions without being aware of it. However in the cases in which introspection was given there is no unanimous tendency. Only two of the subjects are at all consistent in their reports, the others feeling more efficient sometimes under uniform and sometimes under non-uniform lighting. There is no general tendency in the introspective as in the objective results. However the two correlate in about two thirds of the individual cases in which introspection is given—a report that the subject is less efficient under a given system corresponding to a slower reaction time. There are no marked individual differences, *i.e.*, subjects who are consistently superior under uniform or non-uniform conditions. On the whole, considering the few cases in which introspection was forthcoming and its variable character, the principal stress must be placed on the objective results. It is evident that a subjective estimate of the mental state under various conditions of illumination is sometimes unreliable, and furthermore such illuminating conditions frequently influence the reactions without producing any conscious effect at all.

Thus the results obtained in the laboratory tend to corroborate those found on the street although not as marked. Reactions are quicker and a higher degree of attention is maintained under non-uniform illumination. It would seem that the results could be attributed solely to the non-uniformity factor either in the form of the increasing and decreasing intensity or of the shadows of the visual field. External factors such as traffic and extraneous light from store windows were not present in the laboratory. Nor could anything be attributed to the type of lamp or the number and mounting of lamps. Further, when a shadow equipment was used, the brightness of the wall in the corresponding uniform equipment was in some series equated to the average brightness of the former. The results are evidently due to the uniformity factor.

IV. *Correlation of Other Reactions with Auditory Choice*

In the experiments on the street auditory choice reactions were used almost entirely. It is evident that in the laboratory simple auditory reactions as well as auditory choice yield greater efficiency under the non-uniform illumination. Data is available for 20 series in which the two were measured on the same hour with the same subjects. If the individual per cents. of superiority of the non-uniform equipment are ranked for the simple and the choice reactions, the two correlate by the method of rank differences to give a coefficient of .42 ± .13. It is to be noted further that simple visual reactions also tend on the average in the same direction as the others. Scarcely enough series are available in which auditory and visual were measured on the same hour to compute correlation on the basis of individual averages, but the general tendency is manifest.

V. *Voluntary Alertness*

The question was raised in the street experiments as to whether the superiority of reaction under non-uniform illumination was not due to a voluntary alertness produced by the necessity of watching more carefully for obstacles in the darker regions between lamps. It was endeavored in the laboratory to answer this question as to whether voluntary attention to one stimulus under the various illuminating conditions would quicken reaction to another. For this purpose auditory choice reactions were given when the subject's attention was somewhat focused on the patch where the tachistoscopic material was to appear, followed by auditory choice reactions with no exposures intermixed. Series were performed on five different illumination comparisons. 80 trials were given on a comparison. The results for the uniform and the non-uniform equipments were then evaluated separately and the efficiency of auditory choice reactions presented alone compared with that of such reactions with tachistoscopic trials intermixed. The former were found to be superior with the majority of the subjects under all the lighting equipments thus investigated. Considering the totals under the five uniform equipments, 76 per cent. of the series yielded a superiority of the reactions presented without the tachistoscope, with an average superiority (including negative cases) of 5.2 per cent. and the average difference 2.44 the probable error of difference. Considering the totals under the five non-uniform equipments, 76 per cent. of the series likewise yielded a superiority of the reactions presented without the tachistoscope with an average superiority of 7.9 per cent. and an average difference of 2.6 the probable error. Evidently voluntary alertness to one stimulus under the various illuminating conditions does not quicken reaction to another stimulus.

VI. *Shadows vs. Changing Illumination Intensity*

Finally there remains the question as to whether the effect of non-uniformity on the street is due primarily to the changing illumination through which the subject passes as he walks along the street or to the light and dark patches on the street surface. One series was performed to test this directly, auditory choice and simple reactions being given under the 55 sec. dimmer and under the stationary shadows. Choice reactions proved quicker under the dimmer with 3 of the 5 subjects, with an average superiority of 9 per cent. and simple reaction was quicker for 3 subjects under the shadows with the average difference small.

On the whole, non-uniform illumination produced by the dimmer seems slightly superior to that produced by the shadows in its effect upon choice reaction, while the result is reversed with simple reaction, and a combination of dimmer and shadows seems in general to produce better results than either alone. Presumably the effect of non-uniform street illumination upon attention and reactions is due to both factors, the changing illumination intensity through which the individual passes and the dark regions on the street surface.

D. CONCLUSIONS

The results of experiments upon subjects walking under actual conditions of street illumination indicate rather consistently that the auditory choice reaction time is quicker, the attention more alert and the efficiency in motor co-ordination greater under a distinctly non-uniform illumination than under a distinctly uniform. There is no other variable that follows as directly with the results as does the uniformity variable. Sometimes the non-uniform street has more distracting elements in the form of people on the sidewalk or extraneous light from stores, sometimes less; sometimes its average illumination intensity is greater, sometimes less; sometimes it has a different type of illuminating unit, sometimes the same; sometimes it constitutes the first of the series, sometimes the second; sometimes it has fewer lamps with different mounting and spacing, sometimes the same number of lamps with the same mounting and spacing. But under all these conditions it shows a superiority over the uniformly lighted street. When the uniformity factor is a variable in the comparative study of two equipments, there is a marked difference in results, and when it is a constant the difference is slight. Under a distinctly non-uniform street illumination as compared with a uniform, the subjects show in 77 per cent. of the series a superiority in motor co-ordination with an average (including negative values) of 3.6 per cent. superiority; in the tachistoscopic test they are superior in 84 per cent. of the series with an average (including negative values) of 8 per cent.; and in auditory choice reaction every series yields a quicker reaction time with an average of 17 per cent. superiority.

With the problem transferred to the laboratory and non-uniformity produced by slow rhythmic increasing and decreasing of the illumination intensity of the room, or by shadows to break the monotony of the dark wall—conditions designed to parallel respectively the changing illumination intensity through which the individual walks on a non-uniformly lighted street, and the shadows on the street surface—the results tend in general to corroborate those just mentioned. Further, simple visual and auditory reactions correlate with choice reactions in this tendency. The superiority of reactions under non-uniform lighting on the street cannot be attributed to a voluntary alertness produced by the necessity of watching more carefully for obstacles in the darker regions between lamps, for the experiments show that voluntary attention to an expected stimulus under such illuminating conditions does not quicken reaction to another stimulus. Within reasonable limits no definite relation exists between degree of efficiency in reaction and degree of non-uniformity in illumination, the important point being the general superiority of the various degrees of non-uniformity to the uniformity. The superior effect of the non-uniform street lighting seems due to both the changing illumination intensity through which the individual walks and the light and dark regions on the street surface.

Finally the practical implications of the experiment should be borne in mind. Safety on the street depends largely on the ability to apprehend a dangerous situation and carry out quickly the proper psychomotor response. If external conditions produce a lower degree of attention and a retardation of reaction, the danger of such conditions is manifest. The above experiments show that uniform street illumination as compared with a moderately non-uniform, produces such a state in the case of the pedestrian walking through the street. The work in the laboratory with various degrees of non-uniformity would seem to indicate a similar condition in the case of more rapid movement through the street such as that of the driver or chauffeur. Driver and pedestrian alike must make quick decisions and reactions and a difference in reaction time of a few hundredths of a second may avert or precipitate an accident. The conclusion thus seems warranted that, ceteris paribus, non-uniform illumination is more conducive than uniform to safety on the street.

GLOSSARY

Auditory, pertaining to the sense of hearing.
Cm., abbreviation for centimeter.
Correlation, a relationship existing between two sets of measurements, often significant for purposes of prediction.
Mean Variation, the average variation of a set of measurements from the average of those measurements.
Meter Candles, unit of illumination; illumination at a surface one meter distant from a source of one candle power.
Permutation, order of arrangement.
Photometric, pertaining to the measuring of light-intensities.
Presselle, electric key (as a telegraph key).
Probable Error of the Difference, a measure of the reliability or significance of the difference.
Quadrant, quarter of a circle.
Reaction Time, the time involved in executing a simple, co-ordinated movement in response to a simple stimulus-situation.
Signal Magnet, magnet actuating a pointer, pen or other device for recording the time when signals are given.
Stylus, pencil-like marker or tracer.
Tachistoscope, apparatus for brief exposures of stimuli; from the Greek words meaning *quick* and *view*.
Vernier Chronoscope, a chronoscope consisting of two pendulums, one slightly shorter than the other; the difference in the number of swings of the pendulums per minute is known and can be used, under the right conditions, in calculating reaction times.
100 V.D., 100 double vibrations per second.

READING NINETEEN[1]

THE SYNÆSTHESIA OF A BLIND SUBJECT[2]

Editors' Note

An Organismic Theory of Synæsthesia.—The term synæsthesia comes from two Greek words meaning "sensing together," and pertains to such experiences as seeing colors, or tasting, when tones are "heard." There are many varieties of synæsthesia, but all of them have certain characteristics in common. If a person sees a certain color, for example, when a certain tone is sounded, he employs the same color whenever he imagines or *thinks* that tone. This person will also see the same color when he "hears" the tone in his dreams.

Contrary to the prevailing belief, even among psychologists, the colors are not extraneous processes, associated with the qualities of tone by the synæsthetic person. Strange as it may seem the colors are integral parts of the hearing process, and without them the synæsthetic person does not hear the tone. Thus, if a particular person always sees a flash of red when a piano tone is sounded, ignoring the red is the same as ignoring the tone. Contrary also to the popular belief, there is nothing whatever abnormal about synæsthesia. Persons who have it are no different from any one else except that their perceptual processes are different in *appearance;* they perceive the tones just as readily and accurately as any one else. Indeed, they are surprised when they learn that tones heard by other people are not similarly colored.

A perception is always a complex experience. Take the more usual method of perceiving a tone. Suppose a person is blindfolded and a violin note is struck. He could tell instantly that it was a violin tone. But how? What would be the mental processes involved in his perception? He noted the quality of the tone, but that is not all. If he stopped to analyze his experiences carefully enough he would find that he was probably having some kind of a mental picture, a visual image, say of a violin, or perhaps merely the bow poised above a string. Perhaps he repeated to himself the word *violin,* or merely started to repeat it. Perhaps the tone aroused a certain bodily feeling. At any rate an experience was present in addition to the sound, as such, or the observer would not have recognized or even identified it. *This is because the properties of the sound "sensation," as such, depend upon a larger whole,* and that larger whole, in this case, is not alone a matter of sound. The larger whole, in any case, is never confined to one sense department. If it were, it would mean that a part could exist independently of a whole, off by itself and unrelated to anything else, an inconceivable situation. This same principle applies to all kinds of perceptions. In the synæsthetic subject it merely happens that colors, instead of other kinds of processes, may be aspects of the larger whole.

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[1] Parallels *The Science of Psychology,* Chapter XII.

[2] By Raymond H. Wheeler and Thomas D. Cutsforth, University of Kansas. From "The Synæsthesia of a Blind Subject," by R. H. Wheeler, the University of Oregon Publications, Vol. 1, No. 5, May, 1920, 4, 5, 6, 8, 11-12, 17-18, 20-21, 28, 29-37, 38-40, 42-54. By permission.

A perception, then, is a configurational phenomenon and synæsthesia is no exception. Synæsthesia emerges very early in the life of the individual; how early is not known. But it is probable that, in the beginning, mental life is undifferentiated. Colors, sounds, tastes, smells, feelings of contact and muscular movement emerge from this undifferentiated whole into different kinds of configurations which are psychologically known as *qualities*. But they never separate completely from the total pattern out of which they emerge. The total pattern is always present as a "ground" and the quality stands out as a "figure on a ground."

In persons who are not synæsthetic, tones and colors emerge in separate configurations from a common ground. *This emergence comes about as one, total process, however, because the infant's eyes and ears are functioning at the same time.* One is always seeing something while he is listening to tones and is always hearing something when he is looking at objects. It is impossible for a person, not actually blind or deaf, to shut himself entirely off from visual or auditory stimuli.

Accordingly, the theory is here proposed that, in the synæsthetic person, auditory and visual perceptions failed in infancy to emerge as they did in the non-synæsthetic person. *Visual configurations emerged to the exclusion of the auditory, in auditory as well as in visual situations.* The reasons for this curious state of affairs cannot be discussed here, but the theory explains why the synæsthetic subject is deaf to tones when visual processes are not present. What would have turned out to be auditory qualities remained as part of the undifferentiated ground, that is, they never emerged from the total pattern. Color, visual form, movement and position did emerge, and these, for the synæsthetic subject, are various aspects of the configuration which he calls "hearing a sound." Paradoxical as it may seem he hears as well as any one else but he hears visually. Deprive the configuration of its color, or form, or movement, or position, and the entire "auditory" perception is destroyed (R. H. W. and T. D. C.).

One of the earliest obtainable records of synæsthesis was made by Hoffman in 1786. In this record was described the case of a Swiss magistrate and painter who had numerous varieties of colored hearing. No attempt thoroughly to investigate the case appears to have been made. Another early mention of colored hearing is to be found in Goethe's ''Theory of Colors,'' published in 1810. In 1812 Sachs described a very interesting case of an Albino whose synæsthesis included colored vowels, consonants, musical tones, digits, cities, days of the week, dates, epochs in history and certain abstract terms which had to do with phases of human life. Sachs attempted to treat the problem systematically. In his classification of different forms of synæsthesis into acoustic, optical and psychological, according to their origin, we find a very early and noteworthy recognition of both a physiological and a psychological problem in the phenomenon of secondary sensation.

In 1871, Rause reported a case of synæsthesis which was apparently associated in a most remarkable way with a gunshot wound in the lungs. The subject in question had been struck by the bullet in the spina scapulæ and had sustained a second exterior wound in the first intercostal space, where the bullet had found its exit. When a weak solution of lactic acid was poured into the anterior wound the subject could taste the liquid. This accidental discovery led to several very careful tests in which every precaution was taken in order to eliminate the influence of smell and of suggestion. Under these latter conditions, even when the subject had no warning of the stimulus, he tasted weak solutions which were poured into the anterior wound. In fact the patient was able to judge the strength of the solution fairly accurately by means of the intensity of the concomitant taste. Similar tests in the posterior wound failed to arouse any taste sensations.

In 1873 J. A. Nussbaumer reviewed his own case of synæsthesis and also his brother's case, both of which were very complicated and both of which began at the age of four or five years. At this early period in their lives these two brothers argued and fought between themselves as to the colors which they thought belonged to the tones of certain musical instruments. Later, one of the brothers noticed that these same color associations accompanied auditory images in his dreams. Certain peculiarities of the photisms were noticed by the other brother, among which was the localization of his colored photisms either against the forehead or immediately in front of it, instead of in the direction of the sound, as is more frequently the case. He also noticed that at times he could listen to lengthy concerts without the appearance of any colors, while isolated tones were always colored. Nussbaumer attempted to throw light upon the origin of colored hearing by observing the possible effect on the photisms of passing weak electric currents through different portions of the brain. Only negative results were obtained beyond the fact that the current caused faint flashes of color to appear when it was passed through the forehead between the temples! Although this investigation offered no solution to the problem, it stimulated a considerable amount of literature on synæsthesia, first, perhaps, because of its thoroughness as far as it went and second, for the reason that it gave an opportunity for further theoretical speculation. Here the origin of the phenomenon could be traced beyond a doubt to early childhood and here, also, the colored hearing undoubtedly depended upon auditory stimuli.

In 1883 Galton collected a considerable amount of data on various forms of synæsthesis from which he concluded that vowels were more likely to be colored than consonants, and that most types of synæsthesis were inherited. He was perhaps the first to point out that number forms and synæsthesis frequently go hand in hand, that is, many number forms possess permanent colors as well as permanent shapes and outlines.

A series of investigations was begun in 1885 by Holden and continued at intervals until 1906 with his daughter as the subject. Her synæsthesis was tested at the ages of seven, eight, ten, fourteen, seventeen and twenty-one. During this time the only changes that had occurred in the colors were the shift of cream to white as the associate of the digit 8, and the shift of brown to black in the color associated with 10. Twenty-four years after the first tests had been made, colored letters, digits and days of the week had undergone only very minor changes in their brightness qualities.

In 1889, 1892 and 1893 Grüber reported several remarkable cases of synæsthesia, some of which proved of great advantage to the subjects. In one instance so faithful were the associations of colors with digits that the subject was able to perform mathematical operations by means of colors. In this respect this case resembles the one which will be described later by the present writer. In another instance a singer was able to judge the accuracy of pitch by means of the fine differences in shades and tints of his colored tones. Another subject was able in the same way to change the quality of his voice to a very minute degree. Grüber employed the method of measuring projected photisms with a millimeter scale in order to determine the constancy of the colors over long periods of time. His subject in this experiment was a Roumanian who associated colors with digits and diphthongs. The names of the digits were presented to him in the Roumanian language for the reason that his colored hearing was acoustic in origin. The subject was asked to project his photisms upon a white cardboard. The photisms themselves were either circular or oval in shape. The cardboard was surrounded by a colored background and the size of the white interior was varied until the limits of the projected photism corresponded exactly with the limits of the white cardboard. For example, the digit ''2'' was associated with a bright yellow circle. The central cardboard was made in the form of a disc the same shape as the photism. When the subject was asked to project his yellow photism upon the white disc, and the disc was surrounded by a red background, if the disc was too large for the photism there appeared around the edge of the photism, between it and the red background, a band of white, where the disc extended beyond the limits of the photism. If the disc were too small, there appeared on the red background just around the disc a band of orange where the photism extended beyond the limits of the disc and onto the red background. When both the orange and the white bands failed to appear the experimenter knew that the size of the white disc and the size of the photism exactly corresponded. Many of the photisms were oval rather than circular. When different digits were presented to the subject in serial order and according to their value, the vertical diameter of the oval increased with the addition of every syllable in the name of the number. The horizontal diameter of the photisms increased with the addition of every cipher. The diameter of the photisms of the units class was twenty-one millimeters; the photisms of the tens class were twenty-three millimeters in diameter; the hundreds class were twenty-six millimeters; the thousands class were thirty millimeters; the hundreds-thousands were forty-one millimeters; the millions class were forty-eight and so on. If each of these diameters be subtracted from the one previous the differences will read two, three, four, five, six, seven and so on. This remarkable relationship held between the differences in the diameters of photisms of numbers as high as the billions class. A further remarkable feature of this subject's photisms was the fact that one dimension grew vertically and depended upon the phonetic element of the name of the digit; the other dimension grew horizontally and depended upon the arithmetical element or class to which the number belonged. Both diameters increased in the same proportion.

In 1897 Flournoy described an elaborate and striking case of ''personification'' in a young man of nineteen. Names of the days of the week were dramatized as follows: Monday and

Tuesday were a sad young man who held a fore-finger over one eye. This figure designated dark weather. Wednesday was a young man in the act of stealing an object behind him, bending over and extending his arms backward, between his legs. This figure also indicated dark weather. Thursday was a man in the act of turning a knob in the kitchen door on his way to the room beyond. Friday was a man in the act of selling from a wagon the object which was stolen by "Wednesday." Saturday was a man in the act of falling against a door for the sake of amusement. This figure meant clear weather. All the figures were lacking in color but were definite in outline and especially in facial expression. Certain words were colored and others were dramatized. "Autumn" was the same man who represented Monday and Tuesday; "Battle" was a large woman seated at a little bench near a table, laughing. "Shark" was a large horse which stood near a load of hay. Whenever the subject saw or heard the word "shark" this imagery appeared in consciousness before the meaning of the word or before there appeared in consciousness the idea, fish. The tendency to form these associations reached its maximum when the subject was between eight and ten years of age and rapidly declined after the age of twelve. In his earlier life these personifications had been so marked as to dominate consciousness and to require voluntary effort to eliminate them. The subject had always possessed an unusually vivid imagination and he constantly anticipated the subject matter of a conversation or of a book, by means of vivid visual imagery. The remarkable ease and rapidity with which images were linked with words either voluntarily or involuntarily, relevantly or irrelevantly, seemed to be a peculiar trait of the subject's mental make-up. This trait lent itself readily to the development of personifications. It is possible that here we have a case which might be taken as evidence of Ferè's view that synæsthesis depended upon a certain "tone" of the nervous system. In this instance the "tone" might consist of an unusual elasticity of the nervous system, or a like condition.

In 1907 Pierce reported a hitherto undescribed variety of synæsthesia, gustatory audition. The subject was slightly deaf and so anosmic as not to be able to detect the odor of burning coffee. Tastes were recognized not only by means of the elementary taste qualities themselves but by the "feel" of the texture and of the astringency of different foods. Words were associated with these taste and pressure complexes. The following are typical illustrations: Alfred—cornbread in milk; Arthur—small particles of delicate lamb; Emma—pie crust; marry—raisins; hope—celery; loud—boiled potatoes; story—stewed cranberries; who—thick, salty cream. Many of these tastes would appear when the words occurred in conversation or in a lecture to which the subject was listening. The manner in which these words were pronounced influenced the associated tastes thus indicating that the synæsthesis was acoustic in origin. "Light" for example when rapidly pronounced aroused the taste of the soft, juicy lobe of grapes, but if pronounced slowly this same word aroused the taste of chicken gravy. Words had different tastes when pronounced by different individuals. This seemed to indicate that the quality of the sound determined the taste imagery. Her taste associations were so many in number that all of them could not be recalled voluntarily but when the appropriate stimulus was pronounced to her the associated taste never failed to appear. These associations could not be reversed unless the connection between the two sense fields had been remembered and had just been recalled. In testing for the taste equivalents of different words it was the habit of the subject to pronounce the words herself in order to "reinforce the auditory excitation." When in doubt as to the quality of the taste she applied pressure to her cheeks with her fingers. Taste associations were revived most readily when the subject was in a state of natural hunger, a fact which seems to point to a physiological basis. If a lecture or a conversation to which she was listening happened to be just prior to a meal the taste imagery was exceedingly tantalizing. Nonsense syllables, which had been unthought of by the subject before the experiment began, served to evoke taste qualities the first time they were pronounced to her. "Sut" gave mutton fat; "yoz" gave the taste of the white of an egg beaten stiff; "hes" aroused the taste of small particles of mincemeat. Undoubtedly association played a considerable rôle in these latter tests. "Sut" might suggest "suet," a form of fat; "yoz" might suggest the sound of beating up an egg; "hes" sounds something like "hash," etc. Many of the subject's taste images were aroused in the absence of any awareness of the stimulus. For example at one time she was working in a library and while busily engaged in her work she suddenly tasted roast beef. Casting about for a cause, she heard men's voices in an adjacent alcove. The following facts seem to support the conclusion that this case depended upon physiological factors: (1) the associations were constant; (2) the subject was slightly deaf and slightly anosmic; (3) the taste qualities depended upon the rapidity and the quality of the auditory stimuli; (4) many of the taste qualities resisted introspective analysis; (5) the gustatory imagery was clearly localized in the reagent's mouth and was so vivid as to approximate actual taste sensations.

In 1901 and again in 1914 Lemaitre described some very remarkable cases of personification. He found that about three per cent. of adolescents between the ages of twelve and fifteen showed a tendency to endow letters, numerals, or certain words with human traits. One subject experienced considerable difficulty in solving arithmetical problems because of the fact that the digits with which he was working gave rise to vivid and persistent dramatizations. The numeral 8 was a husband whose wife was 4; 3 was a son, twelve years of age; 6 was a timid and feeble daughter; 9 was a wealthy individual, a good friend of 8 but a special friend of 4; 7 was a woman of high social rank and a friend of 8; 5 was a charming lad, very unpretending, who held a unique relationship with each of the members of the family; 1 and 2 were insignificant individuals. The wife, 4, disdained her husband's affections and sought the friendship of 9. Whenever the subject attempted to solve a problem in arithmetic there appeared these personified numerals in human form and outline, standing in a row in the order named above, and in the center of an immense circle. The circular plot of ground was surrounded by a grassy plain. Out on the plain was the habitual residence of the wife, 4, who never left her house except to go in search for her son, 3. At one side of the circle was a steep incline which led upward in one direction to a stone embankment and downward in the opposite direction to a deep crevasse. Then there appeared 4, with her son 3, near this wall. When they approached the wall the numerals combined to make 12. 5 and 3 then appeared on top of the embankment; they were seen to shake hands, whereupon the numbers combined to make 15. At the moment when the numbers combined, the human forms disappeared. Beyond the wall on a level terrace above was situated a stone enclosure where dwelt the numeral 5. After 5 had combined to make 3, it reappeared and went into this stone enclosure. 7 then appeared on the brink of the precipice where 3 was seen to join him. 3 and

7 then combined to make 21. 4 was next seen chasing 7 in the direction of the chasm and when both numerals arrived at the chasm's edge they combined to make 28. In this fashion another drama was staged by 5, 6, 7, 8 and 9, in which, from time to time, different pairs of these numerals combined to make larger figures. Lemaitre thought that the subject formed the habit of dramatizing these numerals when he was very young and in the first stages of learning writing, language and vocabularies.

Myers offered the suggestion that synæsthesia developed in childhood when primary perceptions were still vague and undifferentiated. Synæsthesis was the persistence of this vague and undifferentiated form of perception. Aside from this the subject probably showed a strong tendency to form synæsthetic associations between those sense fields in which there remained vestiges of the old, imperfect and undifferentiated sensory experiences. One of Myers' subjects was the Russian composer, Alexander Scriabine. The composer once tried to have his "Prometheus and Mystery" given with colored lights and odors in order that the visual, auditory, and olfactory sensations might blend into one great harmony and thus produce a greater effect upon the audience. Scriabine's colors were associated with the harmony of music and with musical tonality. Changes in the colors often preceded the change in tonality with which the colors were associated. This made it possible for the composer to judge harmonies by their colors.

In 1917 Jordan reported on the photisms of his son, on whom tests had been made between an interval of five years. Several minor changes had occurred in the color associations, all of which were evidently due, however, to inaccuracies in describing the colors.

In 1918 Alford reported two cases of colored hearing in two male twins, twenty-seven years of age. Both subjects were very much alike in their mental traits. Both were brilliant students in all their studies; both could perform remarkable feats of learning; they liked and disliked the same things; both were fond of poetry, art, and architecture and both were temperamentally emotional, sympathetic, sensitive, shy and modest. On one of the subjects tests of the photisms were made three months apart. Unfortunately the experiment had to be abandoned very early in its course for the reason that the subjects became suspicious and failed to co-operate. 150 proper names were used as stimulus words and in a rather loose manner the subjects were asked to write the names of the colors which the stimulus words aroused. In certain instances the names evoked no color at all; in other instances they evoked several colors among which the dominating color was hard to determine. In most instances the colors were practically as vivid as the real sensations. It was found in this investigation that under conditions of fatigue the colors were more readily stimulated. After the subject sat in an experiment for longer than half an hour there appeared a persisting and confused array of colors which always obscured the appropriate response. A possible indication of reversibility of the associations was found in one of the subjects who could recall several proper names when the stimulus word was a certain color. The responses were the names which always aroused that particular color. Although the colors remained fairly constant over a period of three months, certain stimulus words aroused a color during the first tests and failed to arouse any color in the second test. Conversely certain words which failed to arouse colors in the first test did so in the second. One of the purposes of this investigation was to discover how nearly the associations of the two brothers resembled one another. It was found that the photisms were similar with 67 per cent. of the test words. Harris and his brother had previously reported a similarity in their own cases of only 50 per cent. As the author suggested, the 67 per cent. was probably too high and had the method been more refined and the descriptions of the colors more accurate, the similarities would have been less noticeable. Several indications in this study point to the associative origin of the synæsthesia: (1) the associations were frequently reversible; (2) certain words evoked colors at one time and not at another; (3) the name Brown was "brown." Sue was blue, Fern and Verne were green, Monday was blue (blue Monday) and the like.

SUMMARY OF HISTORICAL REVIEW

No thorough going study has been made of synæsthesis in the blind. Only seven of such cases have been described in the literature which the present writer has been able to obtain. There have appeared no characteristic differences between synæsthesia in the blind and in the seeing. The relationship of synæsthesis in the blind to the onset of blindness if there is any such relationship, has never been worked out. The only theory which has been offered to explain synæsthesis in the blind was offered by Phillipe, who thought that such associations were due to an effort on the part of the subject to retain his memory for colors. If Phillipe's results are to be accepted as representative, synæsthesis would seem to be slightly more prevalent among the blind than among the seeing, suggesting that a certain number of such cases may follow from the fact that the subjects have lost their sight. In those cases of synæsthesis which have developed before the subject became blind no subsequent changes due to blindness have been noticed.

Frequency of Synæsthesis. The frequency of synæsthesis among adults has been found, under varying conditions of age and sex, to range from nine per cent. to fifteen per cent. The frequency among adolescents has been found as high as twenty-five per cent. It seems probable then, that many cases of synæsthesis disappear when the subject reaches maturity.

The following is a list of the different varieties of synæsthesis which have been reported in the literature. They are arranged according to the apparent stimulus. It happens that the list indicates in a general way the order of their frequency, beginning with the first as the most common variety.

I. Of acoustic origin
    A. colored hearing
        1. isolated tones
        2. noises
        3. music
        4. chords
        5. vowels
        6. consonants
        7. words
            a. names of people and other proper names
            b. common parts of speech
            c. days of the week
            d. months of the year
            e. seasons
            f. epochs of history
            g. virtues and other abstract terms
            h. phases of human life
            i. names of tastes
            j. names of odors
        8. digits
        9. dates
    B. gustatory audition
        1. taste equivalents for words
    C. figured audition
        1. geometrical forms associated with words
    D. pain-audition
        1. toothache equivalents for tones of certain quality

- II. Of visual origin
  - A. colored lines
    - 1. serrated lines
    - 2. broken lines
  - B. colored forms and figures
    - 1. geometrical forms, square, circle, rectangle, triangle, etc.
    - 2. colored angles, obtuse, acute
    - 3. colored signs and designs, swastika, etc.
    - 4. colored multiplication, division, addition, subtraction signs
  - C. colored letters, digits, words, etc.
- III. Of gustatory origin
  - A. colored tastes
  - B. colored taste and pressure complexes
  - C. colored taste and odor complexes
- IV. Of olfactory origin
  - A. colored odors
- V. Of tactual origin
  - A. colored pain
  - B. colored pressure
  - C. colored temperature sensations
    - 1. warm
    - 2. cold
    - 3. mixtures
  - D. auditory pain
  - E. auditory temperature sensations
  - F. cutaneous or muscular taste
- VI. Of affective origin
  - A. colored pleasantness and unpleasantness
  - B. colored emotional complexes
- VII. Of possible combined visual and auditory origin
  - A. personification and dramatization of letters, digits, days of the week. (It is more likely, however, that associations play a very important rôle in the origin of this form of synæsthesia.)

Factors Determining the Nature and Attributes of the Secondary Sensations. The hue of color equivalents for sound is generally determined by the quality of the tone. The brightness of the color is determined by the pitch and intensity of the sound. The hue in other instances may be determined either by an emotional accompaniment of the sound sensation or by fortuitous associations. The color equivalents for chords are determined either by a component tone whose color is striking, by the fundamental tone, or by mixing the colors of the component tones. Colors for music are determined either by the key in which the music is written or by the composer's name. The colors for vowels may be determined by the shape of the letter, by the sound of its pronunciation, by affective concomitants, or by peculiar methods of usage of the vowel. The colors for consonants may be determined by the same factors as in the case of vowels, or by the position of the consonant in certain words. Color equivalents for words are determined by the color of accented vowels or consonants, by the colors of letters which are repeated in the word, by the colors of initial letters, by a mixture of the colors of component letters, by affective accompaniments, by the color of a component letter the hue of which is unusually striking, by associating the word with the natural color of the object for which the word stands, etc. The colors for tastes and smells are often determined, apparently, by the natural color of the objects tasted and smelled. On the other hand words for tastes may be colored according to the color of the odor which is commonly associated with such a taste. The colors for digits may depend upon the numerical value of the digit. The colors for dates or large figures may be determined by component digits, by the class of the numeral, or by the number of syllables in the name of the figure. Other conditions which may influence the quality or intensity of secondary sensations are fatigue, emotional experiences and mode of stimulation. Fatigue generally reduces the intensity, clearness and hue of the secondary sensation or lessens its liability of appearance; on the other hand, one investigator found it to have almost the opposite effect. Emotional processes may intensify the secondary sensation or alter its quality, entirely. The mode of stimulation may increase its liability to appear, it may increase its clearness or intensity, or it may alter its quality. In the case of colored hearing differences have been noted in the photisms when each ear has been stimulated separately. The use of counter stimuli has been known to alter the nature of the photisms in colored hearing. In rare instances voluntary efforts to modify or to inhibit secondary sensations have evidently been successful. In rare instances, also, such factors as color-blindness, a condition of hyperæsthesia, and defects in the sense organs have been known to determine the nature and quality of the secondary sensations. More of such determinants of the attributes of secondary sensations might be cited but their lack of verification and uniformity among different individuals makes it hardly worth the while. In fact many of the factors just mentioned have been noted only in isolated cases and it is perhaps questionable whether the reports are always accurate.

Theories of the Origin of Synæsthesis. Four different classes of theories have been advanced from time to time in the literature: the pathological, physiological, association or psychological and a combined physiological and psychological theory. The early attempts of Cornaz and Marcé to attribute colored hearing to an eye defect have been discredited. We have, therefore, the three remaining views. The first noteworthy physiological interpretation was advanced by Nuel and since that time there have been made many supplementations, suggestions of omissions or corrections by different writers. The theory in general includes the important fact that functionally, and perhaps anatomically, there is a close relation between those reflex arcs and brain areas which give rise to the primary and secondary sensations. The functional relation may be one of irradiation, of interaction, of reciprocal innervation, or of a lack of differentiation of function. It may be that the anatomical relationship is one of anastomosis of afferent fibers or of adjacent areas in the brain. In the light of recent theories of irradiation and summation in reflex arcs, the anatomical condition of anastomosis or of tangling of fibers seems to have lost its importance. At any rate such discoveries have had the result of producing a change in the emphasis from structural to functional factors in a theory of synæsthesis.

The following facts support a physiological view: (1) the almost simultaneous presence of secondary with primary sensations at the point of origin; (2) the close proximity of brain areas involved in synæsthesis; (3) the stimulating influence of drugs; (4) the irreversibility of synæsthesis; (5) the destructive influence of fatigue or the reverse effect; (6) the disappearance of synæsthesis in adolescence or in senescence; (7) the dependence of attributes of secondary sensations upon the properties of the stimulus of the primary sensation; (8) the appearance of synæsthesis in early childhood at a time when meaningful associations are not yet formed; (9) the spontaneity and suddenness with which synæsthesis begins.

The first important psychological explanation of synæsthesis was given by Perroud. In general this type of theory may rely upon the alleged fortuitous and simultaneous experiencing of sensations from two different sense fields or upon the habit of constantly visualizing objects in their natural colors. Again, an experience in one sense field may become associated with an experience in another sense field for the reason that both experiences have a common affective concomitant. It may be, also, that colors are deliberately associated with tones, letters and the like, in certain instances, in order that the subject's memory may be improved. In the main, each individual case of synæsthesis must be interpreted differently, if one is to rely upon an association theory. The details of individual cases differ widely as well as the conditions under which each case originates. There are undoubtedly certain forms of synæsthesis which are the outcome of associations. The weakness of an association theory, however, in accounting for the general phenomenon is its inadequacy in covering a majority of all cases, even of the same type. Moreover, the association view, in a last analysis, must rely upon a physiological explanation. The few instances in which the association view seems adequate are: (1) cases which date from some unusual event or experience; (2) photisms which correspond to the colors of the object originally sensed; (3) photisms whose colors are suggested by the position of a letter in a word which designates a certain colored object.

It is clear that such differences of opinion as have existed from time to time between investigators of this problem have been the result first, of generalizations from too few cases; secondly, they have been the result of the vast differences exhibited between different varieties of synæsthesia, and of the great complexity of the phenomenon and, thirdly, to the fact that both physiological and association views have seemed adequate to meet the demands of individual cases while neither view has apparently met the demands of all cases. In such cases as colored odors and tastes, in which the photisms correspond to the natural colors of the objects tasted or smelled, for example, it would be the natural thing on first thought to resort to an association theory, but after all, a physiological view would probably in these instances provide a better explanation than to rely upon the traditional process of association for association is not a cause. Furthermore, to be understood, association must be described in such terms as irradiation or draining of physiological processes. In a last analysis, therefore, the final appeal must be made to a physiological theory.

THE SYNÆSTHESIS OF A BLIND SUBJECT

The subject whose synæsthesis is described in this section of the paper is Thomas D. Cutsforth, at the time of this investigation, a graduate of the University of Oregon. He lost his sight by accident at the age of eleven.

The following is a list of his various forms of synæsthesia and allied phenomena:

1. colored tones; colored auditory imagery of these tones and photisms for the names of these tones
2. colored music; colored auditory imagery of music; photisms for names pertaining to music
3. colored vowels and consonants, covering the entire alphabet
4. colored voices; similar colors for the name of the person whose voice is colored; similar colors for visual imagery of these persons and for verbal imagery of the persons' names
5. colored proper names, cities, towns, states, countries, places, etc.
6. colored cutaneous experiences; photisms of kinæsthetic experiences, touch and kinæsthetic blends
7. colors for abstract terms, theories, beliefs
8. colored tastes and odors; photisms for the names of tastes and odors, which often correspond with the colors for the tastes and odors themselves; colors for auditory or verbal imagery of these names
9. colors for names of the days of the week, months of the year
10. colored digits, dates, telephone numbers, etc., and for auditory and verbal imagery of the same
11. colors for the directions—south, north, east and west; similar colors for auditory and verbal imagery of the words
12. colors for affective experiences and certain emotional complexes
13. photisms in dreams, associated with imagery in other modalities and with visual imagery itself
14. colored number forms, week, month, century and alphabet forms

The following are typical examples of photisms for voices. The left-hand column contains the initials standing for the person whose voice is colored. The descriptions are taken verbatim from the reagent's introspections. On March 9, 1917, and June 9, 1917, the stimulus was either the person's voice or the name of the person given to the reagent shortly after hearing the person's voice. On August 15, 1919, the reagent reported his colors from auditory imagery of the person's voice or from associations aroused by the person's name. In considering this latter series of tests, it should be borne in mind that unless specified in the tables, the reagent had not heard the actual voice for several months and in certain instances not for two years:

	March 9, 1917	*June 9, 1917*	*August 15, 1919*
Wh.	silver grey	whitish silver on dark grey background	dark grey with bright silver gloss
Con.	greyish yellow, old straw color	brownish yellow, color of a ripe English walnut	yellowish grey, poorly saturated, like old beeswax
Sch.	dark bluish black, very muddy, black predominating	dark muddy color, slight greenish tinge	"smudgy" grey, very dark; dusty overcast, tinged with yellowish brown; "negro" color
DeC.	reddish brown	reddish brown, slightly yellowish	a greyish red-brown, poorly saturated

	March 9, 1917	*June 9, 1917*	*August 15, 1919*
Cam.	bright canary yellow	canary yellow	a very bright yellow, golden or canary
Hag.	chocolate brown, very dark	chocolate brown	dark, rich reddish brown, chocolate in appearance
Hay.	dark grey; light streaks running through it	light grey, darker at times, fluctuating in brightness	a rich dark silver grey
Reb.	dark reddish brown, also blue tinge	very dark brown; bluish tinge	dark mahogany; reddish brown; fringe of poorly saturated brown
Zah.	orange	orange, yellowish	a washed-out orange bordering on a poorly saturated yellowish brown
Sha.	maroon	glossy red, maroon color	poorly saturated red; a sort of faded maroon

The following are voices which the reagent has heard repeatedly during the progress of the experiment:

Fat.	steel grey	shiny steel grey	dark dusty grey poorly saturated
Mot.	bluish grey, dove color	smoky grey, bluish tinge	mouse color, not very bright, faded appearance
Sis.	yellow; beeswax	mixture of yellow and pale orange, like new rope	well-saturated yellow but rather dark
Bro.	the black of a faded umbrella	dark dusty brownish black	smudgy black, almost a faded dark grey

The following are colors for voices which the reagent has not heard for periods of three to five years; the photisms are associated with auditory imagery of their voices:

Tof.	black	greenish black, very dark	deep black
Ros.	brown, reddish tinge	brown.	dark reddish brown
Cha.	bluish green with yellow overcast	greyish green, dusky	dark bottle-green
Wes.	a very light dove-grey	dove-grey	silver grey with slight tinge of yellow
Hor.	bright yellow	yellow	very light yellow, bordering on light yellow-tan

High tones arouse small photisms of a faint hue and light tint, merging into light greys as they become higher. Low tones give large, dark photisms, with rarely much hue. Intermediate tones give photisms which are moderately bright and moderately saturated. The size and brightness of the photisms for tones vary also with the intensity of the sound. Loud tones produce colors of lighter tints but do not otherwise alter the hue. Combinations of tones result in a superposition of colors and very rarely in what could be called a mixture. There is never a true neutralization of complementary colors. If actual sounds are used as stimuli, the colors remain in consciousness as long as the reagent attends to the auditory perception. If an auditory image is the stimulus, the photism is unsteady and fluctuates in clearness between a maximum which undoubtedly corresponds closely to the clearness of a visual perception, to a minimum clearness, characteristic of a vague and fleeting visual image.

When several tones are sounded in a chord, or when the reagent is listening to music, colors come and go with kaleidoscopic rapidity. The individual notes seem to retain their own hues and degrees of brightness, but as a background for the changing colors, there appears, for piano music, a changing blanket of blues and greens. For band music this ''blanket'' is more yellowish.

The following are examples of photisms for tones. As before, the stimuli for the different tests are the same:

	February 9, 1917	*August 15, 1919*
piano, high tones	brilliant white	a very bright white
medium tones	bluish grey	grey
low tones	dark grey	almost black
Galton whistle	minute sparks of silver-colored light	streaks of brilliant "brightness" with no color
flute	different shades of blue bordering on violet	faded out blue, higher notes have no color but merely degrees of brightness; not so bright as piano
cornet	beeswax yellow, very bright	yellow; golden color; higher notes almost white; lower notes very pale and darker
full organ	very rich deep black, bluish cast, spots and streaks of brown, with irradiating flames	very deep chestnut brown, approaching a black, dark bluish or greenish cast

The form and outline of colors for sounds vary greatly from a mere thread-like line or streak to a shapeless "cloud" or "blotch." Sustained tones arouse "extended and thread-like" photisms which become longer as the tone is sustained. Short, quick notes, like staccato notes on the piano, arouse small, short streaks of color, depending upon the length of time the tones are sustained and upon the rapidity with which they follow one another. Large, continuous sounds, as for example a factory whistle, arouse widely expanded areas of color, resembling the side of a hedge or other uniform and solid colored mass. If the whistle is broken up into successive blasts, the "hedge-like" photism is likewise broken up into sections. The organ and cornet photisms are shaped like dominoes or similar bar-like objects. These bars always appear in a vertical position. The violin photisms are similarly bar-shaped but appear slanting upward and to the left. Other instruments produce photisms in a horizontal position. Photisms are not stationary in the visual field. As the reagent's attention shifts from the sound stimulus, for example, the photisms become fainter and travel upward, ultimately merging into a grey background.

The photisms for taste and smell are the most difficult of all for the reagent to describe. This is probably due to several reasons. First, the photism seems to be associated now with the taste of an object and now with its odor. Photisms are becoming confused with retained visual memories or visual associations with the actual objects themselves. When the name of a taste is given as the stimulus there may appear one color which will soon give way to another color of an entirely different hue, as the reagent becomes aware of gustatory or olfactory imagery of the object for which the name stands. The following photisms were reported in a series of tests in which the reagent was asked to recall gustatory and olfactory images as the stimuli for the photisms:

	March 9, 1917	*August 15, 1919*
sour	light grey	muddy grey splotch with upper half quite light, lower half much darker
sweet	yellowish green, very light	pale white, very light
salt	reddish brown, the color of fresh iron-rust	white with several specks and streaks of black running through
bitter	yellowish brown, a little browner than beeswax	yellow, brownish tinge

	March 9, 1917	*August 15, 1919*
vinegar	dark green	poorly saturated greenish yellow
quinine	yellow	dark golden yellow
onions	pale buff yellow; the color of manila paper; changed to light blue with olfactory imagery becoming clearer	bright silvery white with occasional streaks of blue
cheese	bright yellowish brown	reddish grey
strawberries	very dark red	rich bluish red, redder than purple
ginger	mahogany red	poorly saturated brown with minute bright specks of white light
ripe peaches	creamy white	light, amber yellow superimposed on a poorly saturated green
pineapple	very dark red	very dark maroon or blackish-red

Introspective reports, similar to the following, indicate that the reagent's color associations for tastes and smells are being confused with possible remnants of visual imagery of the objects themselves. Numerous examples could be given which indicate that the color of the photism has been determined by the color of the object itself. "I perceived the words 'green prune,' and was immediately aware of a small cloud of silvery white light, appearing in front of me and about four feet from my face. I was on the point of reporting 'silvery white' when I became conscious that this was an old visual remembrance of the inside of a green Italian prune. For some little time I then endeavored to get imagery of the taste of green prunes. When the imagery came the photism changed to a dark blue, almost black with silvery white streaks running through it. This I recognized as the color of the taste." The colors for many tastes and odors seem hopelessly mixed with visual associations of the objects themselves. Voluntary efforts to visualize fruits and flowers often result in the arousal of colors which oftentimes resemble the photisms for their tastes or odors. It is probable that the colors associated with tastes and smells are influenced, also, by tactual factors.

The colored associates for cutaneous sensations have undergone considerable change during the period of experimentation. Much of this, however, is probably due to finer analysis later in the experiment. The photisms for cutaneous and kinæsthetic pain are often so vivid as to dominate consciousness to the temporary exclusion of any awareness of the pain itself. The colors for cold, hot and pressure are readily obtained by stimulation of any part of the body. The intensity of cutaneous sensations always determines the saturation of the color and the degree of brightness. The coldness of ice-water, whether drunk or applied to the body gives rise to an exceedingly saturated blue, ranging to black. Hot water, at almost a burning temperature gives rise to photisms of "inky black." Hot liquids are disagreeable to drink for the reason that the reagent imagines himself drinking "ink."

	March 9, 1917	*August 15, 1919*
hot	dark blue, ranging to black	a dark, dirty grey, ranging to black
cold	black	very black, dead, without gloss
pressure	yellow and yellowish brown	yellowish grey, varying degrees of yellow according to intensity of the stimulus
pain	flashes of brilliant light	black

Cutaneous and kinæsthetic blends arouse changing photisms as the blends themselves change. For example when the subject is drying himself after a bath, the body, while wet, is a dark greyish tan which becomes whiter as the drying process continues. Wet tactual impressions are tan or some other shade of brown. A wet portion of the towel, for instance, is darker and browner than the drier portions.

When the reagent attempts to locate places, visually, on the map, they always appear in the same color as is aroused when he hears the name of the place spoken or when he thinks of the place in terms of auditory-vocal-motor imagery of his own voice. When names of places are mentioned orally, to the reagent, the first color to appear is that of the person's voice; when his attention shifts to the name of the place, proper, the photism changes. In the associations of colors with places, there again may be observed the influence of early experiences in the life of the subject. As far back as the reagent can remember, he formed the habit of learning the names of states and their locations on the map by associating them with the colors as they appeared on the map. Evidence of this fact can be found in the subject's tendency to locate places on a map, in colors, whenever the name of the place appears in his consciousness and again in the circumstance that certain states, distributed widely throughout the

United States, appear in the same colors. An attempt was made to obtain the geography from which the reagent first studied that subject but a copy of it is not available. This point, therefore, cannot be verified.

The stimulus in each instance for the following reports was the name of the place in question:

	March 9, 1917	*August 15, 1919*
Oregon	greenish blue, yellow overcast	light yellowish green poorly saturated
Washington	dark bluish green	dark bluish green
California	reddish orange	pale reddish yellow
Texas	very dark drab color	a dirty drab, the color of fir tree trunks
Arizona	color of buck-skin	a bright straw yellow with a tinge of light brown
Utah	dove-grey	dove color with possible light yellowish tinge
Idaho	dove color but lighter than Utah	light smoky blue; faint
Montana	mahogany red	mahogany red, slightly more red than polished mahogany
North and South Dakota	light yellow, tinged with a drab-grey	a beeswax yellow
Wyoming	beeswax yellow	faded orange, red almost gone, mostly a tan yellow
Colorado	dove-grey, darker than Utah	dove-grey, dark, almost a slate blue
Kansas	dark tan, like Arizona although somewhat darker	copper shade, dark yellow or reddish tan
Missouri	very dark red, almost black	very dark brown, almost black
Georgia	very light grey, almost white	almost white, background of grey, dominating color is white
Kentucky	blue, very dark	dark bluish black
Ohio	yellow	buff yellow, almost a bright yellow
Virginia	light green	greyish blue-green
Maryland	blue	navy-blue
Rhode Island	white	color of dirty white house
Chile	red	light reddish brown, dominating color, brown
Switzerland	bluish black	a dead bluish black
France	a red mahogany	very rich brownish red

	March 9, 1917	*August 15, 1919*
Alaska	greenish black as a basic color, bright yellow spots	bright yellow with dark spots
Argentina	tomato red	light tan, streaked and spotted with pink
Ireland	bright yellow	bright yellow, approaching an orange

Color associates for letters and digits are aroused by hearing the name of the letter or digit pronounced, by reading it aloud, or by obtaining tactual impressions in American Braille. The forms of certain of these photisms resemble in shape a figure made by connecting the points of the corresponding Braille letters; others have vague resemblances of the printed letters. As time progresses, however, most all of these photisms tend to approach the size and shape of a small square. They are localized from eighteen inches to about two feet from the reagent's face, which suggests the early influence of visualizing reading material.

The following reports were obtained with the names of the letters or digits as stimuli. Tests with American Braille give practically the same results.

	March 9, 1917	*August 15, 1919*
a	creamy yellow	yellow of rather high degree of saturation, between orange and lemon
b	jet black	very rich and dark bluish black sometimes slightly glossy, almost the black of crow feathers
c	dark silvery grey	light dusty color, too light for smoke or dove color but more like dust on new rubber boots
d	bluish grey	color of bottle-green
e	dull white	a brightness, just light
f	reddish brown	dull brown, poorly saturated, like a faded brown house
g	glossy white	white, or slightly greenish white
h	a foggy, greenish grey	dark, dirty grey, like weather-beaten boards
i	very brilliant white	white, brighter than "g" but not as bright as "e"
j	creamy yellow, different in shape from "a"	the light yellowish grey of boards, turning old
k	brownish-orange	copper-yellow, carrot-color
l	faded blue	smoky blue, rather pale
m	moderately saturated blue	brown, with yellow tinge, old fallen leaves
n	very light blue, lighter than "l" and "m"	same as "m" but lighter and less saturated

	March 9, 1917	*August 15, 1919*
o	dark muddy color	smutty black, like an old dingy stove
p	pale bluish green	light greenish-blue, washed out in appearance, something like the color of blue-flag
q	dead, smoky black	very dark greenish black
r	black, like "b," but different in shape	the black of a black felt hat
s	light yellow	either a very light yellow or creamy white, color of oil-lamp flame
t	very dark brown	a light chocolate brown
u	tan yellow	greyish yellow, poorly saturated
v	very pale blue	smoky blue, poorly saturated, very light
w	slate grey, larger than "q" and not as dark as "b" and "r"	smudgy, smutty black, like poorly blackened shoes
x	dark brown, two shades	rich black with fringe of red
y	reddish blue, almost violet	color of faded black ink
z	very dark purple	very dark, almost black, tinge of red, not as red as "x"

The colors for digits are similarly fixed over long periods of time. Numbers in the tens class are more richly colored than those in the units class. The photisms for the hundreds class are very poorly saturated. Colors for the thousands class appear on a dark grey background; those for the millions class appear on a bright yellow background. Complex numbers of several digits possess the colors of the component digits and the colors are super-imposed upon a background which corresponds to the class indicated by the number of digits. For example, 85 has two colors, the yellowish grey for the 8 and the bluish black for the 5; the colors appear in bars, side by side. 851 has three colors, 8531 has four, and so on.

As a rule, the letters whose sounds are louder, more brilliant, or resonant, are brighter and lighter than the others. The explosive letters, such as b, g, k, p, t, have the greater saturation.

The following are the reagent's reports of his photisms for directions of the compass. The stimuli were the names of the directions. The same colors appear when the reagent is conscious of attending to the same compass directions, whether or not verbal imagery is present in his consciousness.

	March 9, 1917	*August 15, 1919*
north	black	very rich black, or bluish black
south	yellow	sand-colored yellow
east	red	a reddish brown, bordering on reddish orange
west	brown	little bottle greenish-brown

Foreign words which have become Anglicized and English words derived from foreign roots are recognized, by the subject, from their colors. The following are illustrations:

	March 9, 1917	*August 15, 1919*
German words	dark buff	dark brown, very dark
French words	reddish brown	rich but light brown
Latin words	very dark blue	dark bluish black, the blue very rich
Greek words	white, with faint tinge of blue	a smoky, silvery brightness, bordering on bright grey, almost white

automobile—light blue, chauffeur—reddish brown, conduct—"con" dark blue with "duct" a shade of brown, pantheist—light steel blue.

The following are color associates which have originated since the subject entered college. The stimuli were the names for different theories or abstract terms, standing for different lines of thought.

	March 9, 1917	*August 15, 1919*
"Great man" theory in history	very dark brown	dark yellowish grey
"Economic interrelation" theory in history	pale green	poorly saturated and light green
"geographic" theory in history	dark green	very dark bluish green
Weissmann theory	dark, glossy, rich reddish brown	strongly saturated reddish brown
behaviorism	drab color	a slaty, bluish black
geneticism	white	a very light, dusty, slightly greenish white

The following are *O*'s reports of colors which are associated with affective and emotional experiences. Tests indicated that the colors which were aroused by associating the name of the experience with the experience itself were as faithful as the colors which appear in the actual presence of the emotion.

	March 9, 1917	*August 15, 1919*
pleasantness	yellow	light yellow
unpleasantness	blue-grey	dull, dark grey
anger	very intense bluish black ranging to "jet" black	black when there is no muscular movement; action arouses light streaks which run through the black
joy	silvery-grey, ranging to white, according to intensity	yellowish-grey; silver cast

Tones, voices, tastes and odors have been colored as far back as *O* can remember—to approximately five years of age. Digits first became colored when he began to study arithmetic, at the age of seven. It was at this period, also, that number forms originated. Letters, however, were not colored until the subject began to study the point alphabet at the age of eleven, just after becoming blind. Places, such as states and countries, were first colored, as far as the reagent can remember, when he began to study geography in the intermediate grades at school. He derived much help from these colored associates before and after blindness set in. Photisms for systems of abstract thought, theories, etc., did not appear until the subject entered college. Each type of color association developed spontaneously, but after its appearance, was voluntarily practiced for the assistance it gave the subject in learning and in reasoning.

Interesting light upon the behavior of these photisms may be found by a reference to the genetic stages of the process of recognizing. When a new sound, whether a person's voice or a nonsense syllable is first heard, a color immediately appears, determined in part by the pitch of the sound, an affective component, if any, or the resemblance of the sound to a familiar one which already has a color. An interesting illustration of this immediate appearance of the color is to be found in the fact that the names of towns, areas, localities, etc., in the war zones aroused vivid colors on hearing them for the first time. A second hearing of the same name never failed to evoke the appropriate color. As soon as the subject becomes so familiar with a new name or voice as to be able to get an auditory or verbal image of it, this imagery is appropriately colored. The appearance of the color is a large factor in bringing familiarity with the recalled experience. Subsequently, in the process of forgetting, the vocal-motor or auditory imagery is the first factor to disappear, leaving a feeling of familiarity with its component motor attitude, together with the colored "blanket." For example, after the name of a certain French village in the war territory had been forgotten there was still left the appropriate color for the name of the village. The hue of the color, and the shape and size of the photism then meant to the subject that particular French village. Still later, in the process of forgetting, all associations such as remembrances of events that had taken place in this particular French village disappeared, leaving, in the reagent's recall, only the colored "blanket" which was aroused by the general situation, "village in France" as the stimulus. The same procedure, in forgetting, is characteristic of other subject matter, such as history, geography, or what not. Still later, in the process of forgetting, the photism itself may become fainter, less saturated, until none of its attributes can be recalled. Many times colors appear in the reagent's consciousness which have lost their original associates and can be identified only as the color for a "certain person's voice," a "certain place" whose identity has been forgotten. Thus, in the process of learning and forgetting the objective stimulus appears at the outset with its color. The color is ultimately the last detail of the experience to disappear from the reagent's memory.

The case thus reported presents several peculiar characteristics which have not hitherto been discussed at length in literature nor have they been subjected to prolonged and intensive laboratory tests. Color associations similar to those of waking life appeared in the dreams of one of the Nussbaumer brothers. Our reagent reports similar phenomena. Galton noted that in certain instances number forms had permanent colors as well as numbers. Our reagent has colored number forms in which the numbers are not visualized but are represented by colors or by different shades of brightness. Many investigators have noted that color associations are not confined to the field of perception alone but are also found in the field of imagery. Tendencies to dramatize or to "personify" letters, words, numbers and the like may be included in this category as well as photisms or other synæsthetic phenomena in thinking and in dreams. In the case of our observer not only are auditory, tactual and other sensory experiences associated with colors but images of these experiences are associated with identically the same colors. Moreover, the names for different tones, the names of persons whose voices are colored and the names of other sensory experiences suffice, in most instances, to arouse the same colors as do the actual sensory experiences themselves. In both intances of imagery and of names only one sensory experience is required before the color is likewise associated, apparently permanently, with the corresponding image or name. It seems probable, therefore, that the same physiological condition which would explain synæsthesia in the field of perception might also apply in the field of imagery.

Since synæsthetic phenomena in the field of imagery to all appearances reveal the same characteristics as do the same phenomena in the field of perception, to regard synæsthesia as belonging to the latter field alone is an error. Not all investigators of this problem have reported the fact that the associated image appears in processes of imagining, thinking, etc. A sufficient number of positive cases have been reported, however, to lead one to believe that if all cases had been thoroughly analyzed the same synæsthetic phenomena would have been found in the field of imagery as well as in the field of perception. Several other considerations point to the conclusion that synæsthesis is not a phenomenon of perception alone. First, the color or other associated image may outlive the primary experience in the process of forgetting. Secondly, the photism or associated image may appear in connection with a subliminal stimulus in the absence of the primary sensation. Thirdly, under conditions of fatigue, nervous excitement or of drug taking, the behavior of the secondary image is altered to a greater extent than the behavior of the primary experience. Fourthly, the secondary image shows further, a greater degree of independence of the primary experience than most investigators have apparently assumed, in that when colors for two tones are complementary, these colors tend to follow the first law of color mixture. One investigator found that the photisms tended to follow the second law of color mixture.

GLOSSARY

Acoustic, auditory; pertaining to hearing.
Anastomosis, a growing together or entangling.
Associations, colored aspects of perceptions of tones (as used).
Cutaneous, pertaining to touch; pertaining to the skin.
First Intercostal Space, space between the two upper ribs.
Gustatory, pertaining to taste.

IMAGE, simple imaginal experience of seeing, hearing or feeling, etc.; incomplete sensory process.

IRRADIATION, spreading of nerve currents in all possible directions from a given source of excitation or stimulation.

KINÆSTHETIC BLENDS, perceptions of hardness, softness, wetness, dryness that depend in part upon muscular movements of exploration and in part upon pressure experience and temperature.

NUMBER FORMS, stereotyped and persistent patterns, designs or figures visualized in color, or with numbers located in different parts of the pattern.

O, observer, subject.

OLFACTORY, pertaining to smell.

PHOTISMS, visual phenomena involved in hearing tones or in having other non-visual perceptions.

REAGENT, subject; reactor, observer.

RECIPROCAL INNERVATION, the mutual excitatory and inhibitory effect of two parts of the nervous system; pertains also to innervation of the muscles.

SECONDARY SENSATION, the color or other so-called "associate" supposed to accompany the "primary" or main experience in synæsthesia; in reality it is not a secondary phenomenon but the primary one.

SPINA SCAPULÆ, projections of the shoulder blades.

SUMMATION, accumulation of the effects of stimulation.

STRUCTURAL, anatomical; pertaining to parts.

SYNÆSTHESIS, synæsthesia.

GROUP VI

OBSERVATIONAL BEHAVIOR

READING TWENTY[1]

THE NATURE AND PROBLEM OF PERCEPTION[2]

Editor's Note

Previous theories of perception, like those of other aspects of behavior, rest upon the assumption that the parts explain the whole. The parts that supposedly accounted for perception are sensations and images. Sensations or sensory processes are the simplest items into which experience can be broken down. In the field of vision they are colors and brightness; in the field of audition they are tones and noises; in the field of taste, they are the qualities of salt, sour, sweet, and bitter; in the field of smell there are several, the exact number undetermined, such as spicy, fragrant, putrid and burnt odors; in the field of touch they are pain, pressure, warmth and cold; in the field pertaining to bodily movement and position they are muscular sensations, strain sensations from the tendons and articular sensations from the joints (those of this group are known as kinæsthetic sensations), also swimming sensations and dizziness, from the organ of equilibrium situated near the organ of hearing.

Images have been defined as reproduced, or cortically aroused sensations, to differentiate them from ordinary sensations which definitely depend upon the functioning of the sense organs. The imagining of any of the sensory processes just enumerated would be an example of an image. One can visualize colors, hear tones "in his mind's ear," imagine tastes, smells, bodily movement, and so on.

The fashion in which these parts, or "elements" were supposed to combine into a perception is explained by Dr. Helson, who has brought together the outstanding theories that heretofore have not been available in one article. He then states the general features of an organismic theory and presents typical facts to substantiate it.

The Nature and Problem of Perception

When we hear our name called, watch a moving picture, feel warm or cold, taste the flavor of a fruit, or react consciously to any object or event whatsoever, we are said to be perceiving or to have perceptions. It is almost self-evident that whenever we are perceiving, even though we may be quite passive, *e.g.*, when a loud sound forces itself upon us, we are *doing* something. The organism is active in every case of perceiving. Sometimes the organic activities involved in an act of perception are gross, patent to any onlooker, as in the case when we turn our eyes to see who has just entered the room, or they may be so minute, and restricted to inner movements of the organism, that no overt changes can be detected by an onlooker. Extreme cases of the latter type of perceiving are common in so-called *catatonic stupor* where the patient makes no response to any kind of stimulus, not even blinking an eyelid, yet nevertheless after possibly months of this negativism, tells about everything that went on during the period of apparent stupor. It will be seen that some of the most interesting and difficult problems of perception arise where overt responses can hardly be detected even with the most delicate instruments at our command. In such cases the physiological processes involved in the act of perceiving have to be sought in the functioning of internal organs and glands and the central nervous system, particularly the higher centers. From all of this it follows that those theories of perception which are merely descriptive and give the impression that in perception we have merely a kaleidoscopic play of sensations and images, or a series of conditioned reflexes in which the organism is passive, cannot be true. Perceiving always involves an active, selective attitude on the part of the organism. With this in mind we can proceed to a treatment of perception which satisfies the most rigorous canons of science.

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[1] Parallels *The Science of Psychology*, Chapter XII or XIII.

[2] By Harry Helson, Bryn Mawr College. Written specially for *Readings in Psychology.*

The problems we have to solve regarding the perceptual processes are no different from those that confront us in the many other fields of psychology—indeed they are very closely related to the problems of learning, memory, intelligence and action. What are, then, the problems science seeks to answer? In scientific procedure we believe that if we can state the necessary and sufficient conditions governing a given object of study, then we have gone a long way toward being able to answer the many questions that may be asked concerning it. We may even be able to predict what will happen under both known or tried conditions and under unknown, untried, conditions. While perfect prediction may be impossible, nevertheless prediction furnishes us with the only possible test of our explanations and theories. So in dealing with perception we must be able to say what will happen under any set of conditions. If we cannot do this, then we have a problem. Take, for example, this question:

"What will the glowing end of a cigarette look like under *red* light?" In ordinary light we know that it will look orange-red. If the reader tries this little experiment he will be most surprised at the result, for the cigarette, as the writer has discovered, looks *green.* To explain this we either have recourse to theories of vision which are based upon other experiments or we make a large number of experiments in which we vary the conditions in order to explain this queer result. So far as this problem goes, after many experiments the writer has and knows of no explanation. Here is a chance for further work on a problem in perception. And whatever helps us to understand the activities of perception will help us to understand, predict, and control, numerous other facts of behavior.

Importance of Theories of Perception

Just as theories are important regarding the nature of the atom, the process of evolution, and the nature of intelligence and learning, so will our theory of perception influence our outlook upon a large number of facts. Theories determine what we shall look for, how we regard our findings, and our general attitude toward the larger questions to which every theory in science points. Since, as we shall show before the end of our discussion, perception is intimately bound up with all the activities of the organism, our theory of perception will have an important bearing upon our conception of other activities which may seem more closely related to practical life, although we must not forget that the perceptual processes play a most important part in everyday life, so important in fact that most of the so-called applied psychologists are really concerned with testing perceptual abilities in vision, hearing, and touch. Furthermore, one of the quickest ways to get a survey of a large body of facts and to know how to place and handle facts is through a consideration of fundamental theories. This has long been known by physicists and mathematicians who devote a large space to presentation of important theories. Thus a recent book on mathematical physics (*Foundations of Potential Theory* by O. D. Kellogg) has a statement to this effect near the beginning: This whole book is an exposition of practically one theory. And a recent book in psychology (*Gestalt Psychology* by Wolfgang Köhler) which is generally conceded to be an important contribution is mainly a discussion of theories. We must remember that slight changes or differences in theory, like the slight turnings of the rudder on an ocean liner, make, in the end, a big difference in where we finally land. Hence the importance of theories and our understanding of them.

The Context Theory. We begin our discussion of the theories of perception with this theory because it is stated the most clearly and is the most widely held theory in this country. It was advanced by the late Professor E. B. Titchener (*A Beginner's Psychology*) and has dominated most of the textbook accounts of perception. This theory is important because it is based upon certain very definite and rigid views as to what a psychological treatment of perception should be; also because of its apparent simplicity and applicability. It begins with the assumption that the real data of psychology are sensations which are the psychological atoms corresponding to the chemical atoms in chemistry. The task of the psychologist is to reduce all complex perceptual processes to their simpler sensational constituents. Perceptions have meaning, that is, they refer to objects and events beyond sensation and since Professor Titchener did not believe that science deals with meanings, it is the task of psychology to reduce the meanings to real, existential units—sensations. When two or more sensations get together, their context gives rise to meaning. Meaning is context. Enumerate the items in the context and the resulting description is supposed to contain only the psychological atoms making the meaning, after the analogy of the atoms of chemistry which form compounds. The definition of perception, according to this theory, is: a perception consists of a core of sensations furnished by the stimulus and a context or fringe of secondary sensations or images furnished either by accessory stimuli or past experience.

As an illustration of this theory, take my perception of a chair. I see a chair. But what I see is a complex psychological object composed of many sensations and images, different colors, images of sitting in it, of strain, etc. The chair as such is a meaning. Only the sensations of color, images of strain or kinæsthesis, "really" are psychological. Anything else is meaning arising from the juxtaposition of sensations and images. In this way a number of difficult problems are easily solved. These problems and their solution, according to the context theory, are as follows:

(A) Individual differences with respect to a given stimulus.

Suppose I take out my fountain pen and show it to a group: to me it is "my" pen and arouses a wholly different mental set and attitude from that aroused in others. The explanation here is that because different secondary sensations and images (due to my past experience with this pen) accrue to the core of sensations aroused by the stimulus, the *meaning* of the pen is different for you and for me.

(B) Differences in perception where only one individual is concerned. This includes the so-called illusory perceptions or illusions. For example, if two lines are drawn parallel on a sheet of paper they will appear to converge at one end and diverge at the other if they are cross-hatched, as in the Zöllner figure (see A, Figure 22). According to the context theory the secondary sensations aroused by the criss-cross lines change the meaning or, in this case, the core itself. Hence, whenever a given stimulus does not appear as we should expect it to, the context theory, in order to save the assumption that a given stimulus always arouses the same core of sensations, asserts that the context or fringe is different, giving rise to a different meaning. All unexpected modifications in perception find their explanation through being relegated to the domain of meaning—a very rich storehouse for all kinds of things. Since anything may be a meaning, all difficult problems can be readily explained by appealing to the "meaning" which the constant sensations are supposed to evoke. The list of Titchenerian sensations, like the chemical elements, is comparatively small, so what does not reduce to these elements must be a meaning.

Before we give our assent to this theory and its way of explaining the facts let us see what assumptions it really makes, and if we are willing to accept them. The theory seems very plausible and appeals to us because of its simplicity and easy disposal of difficult problems. But in science the shortest way home is often the longest way round. We shall criticize these assumptions as we state them in order that each point may be evaluated as we proceed. The context theory assumes that:

(1) A distinction can be drawn between meaning on the one hand and psychological existence or process on the other. But since the most elementary sensation means something, to separate meaning and process fails at the very first attempt to do so. In fact, the sensation is such a complex meaning that without special training and a knowledge of what to look for, observers fail to find it. This fact was recognized by Professor Titchener

when, in later years, he came to regard the elementary unit of mind not as the sensation but the units or dimensions composing it, *viz.*, quality, extensity, intensity, attensity, and protensity. This reduction made the psychological element an even poorer unit than the sensation, for it came from an even more abstract way of thinking than the sensation. If it is agreed that we never experience a quality, an intensity, or any of the other dimensions apart from a larger whole, then to neglect the accompanying features means that we have selected certain aspects of the experience for our theory and left out others which may be just as important.

(2) Psychological analysis consists in merely describing or reducing complex processes in terms of simpler. The context theory was admittedly a theory designed to fit into a "descriptive" psychology. Can one believe that my perception of what we may call a "friendly face" is adequately described as being merely a concatenation of so many hues, tints, and chromas, having such and such an extent, intensity, clearness and lasting for so many seconds? Such a description hardly does justice either to what we see or to our reaction to the seen object. There is nothing in this analysis to distinguish it from the analysis of an "angry face" for the latter will also be stated in terms of the dimensions we have given above. If an analysis is to mean anything, it should enable us to indicate what is *different* in the two cases, what gives rise to "friendliness" or "anger" in a face and how my reactions may be controlled. If it is said that the aim in the context theory is merely to "describe" a friendly face psychologically, our answer must be that without fulfilling the additional demands we have imposed, description is child's play and not science.

(3) The second assumption, just noted, entails a third which is that psychological analysis, or description, consists in a reduction of *mental* states into *mental* elements. Even those psychologists who have been interested in *behavior* have at times been willing to grant that it is possible to have a science of *mental* phenomena. On this point the writer would agree with the extreme behaviorists that no purely mental science is possible, although his reasons for believing so differ radically from the arguments advanced by the behaviorists. We point out that in perceiving the organism is doing something. Unless one holds to *pure mental acts*, something very few psychologists believe in today, then he is obliged to admit that physical, bio-

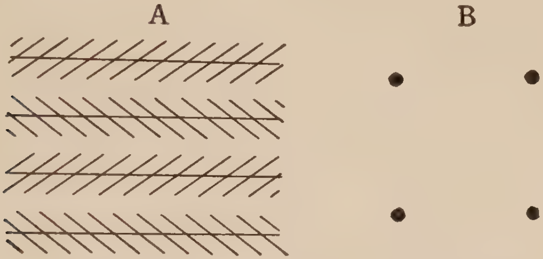
logical, and physiological conditions enter into every case of perception. A description in terms of mental process *only* is at once seen to be wholly inadequate to the task we have set ourselves, the analysis of perception. In my perception of the chair, for example, the chair is composed of physical entities, the colors will be a function of the wave-lengths coming from them as well as certain photochemical processes in my eye. My inheritance, especially if I am color-blind, will also play a part in determining what I see, and finally, the total state of my nervous system at the time of looking. If we wish, as we may for a moment, to neglect all of the physical conditions outside the brain, we must say that perception is bound up with dynamic processes in the brain so intimately that its separation from them is absolutely impossible. There is no boundary line between the physical, so-called, and the mental. The mental processes turn out to be mere items in a larger causally-coherent whole which embraces physical, physiological and biological conditions. These cannot be neglected in our theory of perception.

(4) The context theory assumes, further, that a given stimulus always gives rise to a given *core* of sensations, for it is said that the difference between your perception of ''my'' pen and mine lies not in the primary sensations aroused by the stimulus but in the secondary sensations and images accruing to the core. We may test this assumption by appealing to another illustration used by Professor Titchener himself. Suppose I look at my lawn and report that it ''looks'' green. If I look at it through a narrow tube, as observers did in the Cornell laboratory some years ago, all I can see now is a bit of green in one place, brown in another, yellow in another, green again, and so on. How explain the perception of uniform green which I reported in my natural mode of observing? Professor Titchener's answer was that to the sensations of different colors an image was added (memory overlay) to give the appearance or meaning of uniform green. The constancy hypothesis assumes that both in my natural observation and in looking at the lawn through the tube, the sensations were the same, the difference in what was seen being due to context or what was added to the core in the first case.

Since the final court of appeal in science must be actually observed fact, let us go back to the first observation. Is it true that I then saw little bits of different colors? If the reader will go back a few sentences and trust what is there said he will find that what was seen was a *uniform green*, certainly not what was seen with the tube later on. Nor is it possible to detect the memory color that is supposed to ''overlay'' the different bits of color seen through the tube. What warrant have we for asserting that what is observed under one set of conditions (with the tube) is the only valid method of observing? Why should we give the bits of color seen through the tube a more real status than the uniform green seen in natural observation? To save the assumption that a given stimulus always evokes the same core of sensations? That would be tantamount to losing sight of the most interesting and difficult problems with which we have to deal in the psychology of perception.

(5) The last assumption made by the context theory which we shall mention is that it is possible to explain the complex by merely adding simples together, somewhat after the analogy of building water from hydrogen and oxygen. Mere addition is the simplest way to build complex formations out of simple units. One can no more explain a complex perception by enumerating the sensations and images in it or even describe it adequately, than a chemist can take two atoms of hydrogen and join them to one of oxygen and by that procedure manufacture H_2O. We must find out how things really go together, discover the law of combination and the real atoms in the complex before we can claim to have a satisfactory theory of complex processes. Analysis and synthesis in science are not always of the direct simple kind the context theory leads us to suppose they are. In order to analyze a perception properly we must proceed by way of the conditions under which the perception comes into being. Analysis may not disclose sensations and images at all, but processes of an entirely different character. We may even be obliged to postulate neurological processes before we can gain any real insight into the essential mechanisms underlying perception.

The Production Theory. This theory is similar in some respects to the theory we have just been discussing. We can best understand it, and the main classes of facts it was designed to explain, by reference to a simple illustration. Suppose we gaze at the four dots as shown in B, Figure 22. What do we see? We may see a number of different things: a square, two horizontal lines, two vertical lines, two oblique lines, a triangle with a dot outside, and many other patterns. This is but one of the many so-called ''ambiguous'' figures, the Necker cube, staircase illusion, and, in general, the illusions of reversible perceptive falling in this class. Sometimes it seems as if we can form a given figure at will, at other times a given figure may persist in spite of anything we can do until it suddenly changes into something else. If we call the figure seen a "complex" or a configuration, the question becomes: "To what is due the formation of complexes in perception?" The answer to this question appears in the "Production" theory of the famous Austrian psychologist Meinong, and his eminent pupils and successors, Witasek, Höfler, and Benussi. They assumed that the dots composing the figures seen are sensations, as well as the whiteness of the paper on which they appear. A given stimulus always gives rise to the same sensation, but the same group of sensations may be *combined* in different ways to *produce* different complexes. The act of producing these different figures, as well as the mechanism by which it is done, is called *production process* (*Produktionvorgang*). The production process acts spontaneously without being subject to any known laws, although the sensations upon which it acts are amenable to law. It seems as if production processes can produce almost anything out of sensations like the magician who takes rabbits, ribbons and pigeons from a hat.



Zöllner's lines

Dot figure

FIGURE 22.

It is not very difficult to criticize a theory like this. The student can easily do it himself. It will be noticed that practically the same assumptions are made in this theory as we find are resident in the context theory, particularly numbers 1, 3, 4, and 5 which we have just discussed. It will therefore not be necessary to point them out, with the necessary changes, in this theory. We need merely say a word or two bearing upon the peculiarities of this theory as opposed to other theories of perception.

In the first place it is not true that a given stimulus always gives rise univocally to a given sensation. Not even such simple experiences as sensations, if they exist, will be found to behave so simply. What a given sensory quality will be will depend upon the previous state of the end-organ which mediates the quality, upon the total pattern in which the stimulus is enmeshed, and the time during which it acts, to mention only a few of the factors. Thus if one hand is held in water of 20° C and the other in 45° C and then both are plunged into water of 33° C, to the former the 33° C will seem hot, to the latter, cold. If the hands are held for a time in 33° C, adaptation sets in and the water feels lukewarm, while after a time no temperature sensations are present at all. It is therefore unsafe to assume that even sensations obey the constancy hypothesis.

Secondly we must note, in view of what has been said above, that if so-called sensations are no more constant than the more complex processes, both must be treated alike and are either amenable to law or not. The only course open to scientific psychology must be that even though we do not know the laws governing difficult cases, nevertheless all is subject to law and order which must be discovered. As a matter of fact we do have many laws in psychology as the student has already learned. Even though not a single bit of color in our visual field, not a single sound, pressure, temperature spot or taste but what is influenced by neighboring factors, we still have a respectable body of knowledge concerning the various conditions producing changes in state of both simple and complex processes. So far as the unpredictability of what will be seen or heard when a complex situation confronts the organism is concerned, we are actually better able to say what will happen in many complex cases than when simple stimuli are present. We know that a child will see a face and react to it as such, distinguishing its parents from other people, long before we have any evidence that a differential response to bits of color can be made. Compared with the child's ability to recognize objects and people, the subject of its sensory capacities remains a moot question. In these instances the perception of complexes appears to be more certain and acquired earlier than the perception of elementary sensory qualities. And even the exceptional cases mentioned by the *production* theorists seem to be well on the way toward conforming to known laws.

The Attention Theories. Since the word "attention" has been used in a number of different senses by various writers we shall have to discuss at least two types of theory which may properly be called "attention" theories of perception. We shall take first the theory which regards attention as an active process and then consider the theory as it employs attention in the sense of "clearness" or in a purely descriptive manner.

G. E. Müller, dean of German psychologists, is the foremost representative of the group which regards attention as an active process playing somewhat the same rôle in perception that the "production process" plays in the theory just discussed. His views may be found in two very small books not translated into English: *Komplextheorie und Gestalttheorie*, 1923, and *Abriss der Psychologie*, 1924. Just as in the other theories, again we find the assumption that the primary data of perception are sensations which are united to form complexes. Only here it is "attention," bolstered by other factors which we shall give, which combines and separates sensory elements. When we see a square, hear a chord, or feel a pattern of qualities upon the skin, simultaneous attention, or one act of attention, is responsible. In the case of temporally extended perceptions, like the hearing of a melody and the perception of the words in a sentence, successive acts of attention are required to knit the sensations into meaningful wholes. But there seem to be instances where grouping occurs naturally or without effort on our part. Here other factors are responsible which Müller enumerates as follows:

1. Spatial contiguity: objects near together in space are connected more easily than more distant ones.
2. Similarity and equality of the constituent elements favor the formation of complexes.
3. Impressiveness of stimulus: anything making for impressiveness tends to stand out in attention.
4. Symmetry: symmetrical properties of spatial objects help the parts to cohere and thus to stand out in attention. Thus if two curved lines are symmetrical about a common axis there is a tendency to perceive them as a unit, especially when the axis of symmetry coincides with the median plane of the head.
5. Contours: common contours make for unity. If two figures having little to do with each other are surrounded by a line

(contour) they will form a better unit than before: the common contour brings them together. Furthermore, a figure with a definite contour is a better unit than one with a poorer outline.

In so far as this theory recognizes that the organism is active in perception and that dynamic processes underlie perceptual activities we are heartily in sympathy with it. While Professor Titchener and a number of other eminent psychologists have objected to Müller's use of the concept of attention as an activity, preferring to regard attention descriptively (as clearness), it did lead to a physiological theory of attention, which we shall not discuss, and has much to commend it in suggesting a dynamics of perception.

But Müller was not consistent in his use of the term attention and, clinging to the view that attention unites sensations, finally landed in a *reductio ad absurdum*. He begins by regarding attention as an active cause uniting sensations into complexes and then, as in 2 and 5 above, appears to regard attention as an effect when he speaks of similarity and contours as making things stand out in attention. The theory seems to be absolutely untenable when it forces us to assume that in the perception of such a word as "CAT" three acts of attention are necessary to perceive the whole word, while in a long word like "AMALGAMATION" twelve acts of attention give us the unified impression. Finally, we may note that not even the various uses already given suffice to explain all the facts, so Müller finally relies upon *past experience* and *habit* to help him out of certain difficulties in his theory.

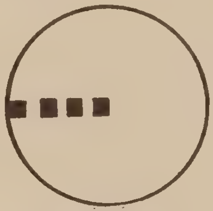


FIGURE 23

The activity theory of attention has been used to explain anomalies of perception when no other explanation seemed possible. Thus it has been held that a sound is louder when we attend to it than when we do not; if we fail to recall an experience it is often said that we did not attend to the original experience. On the other hand, Professor Knight Dunlap has pointed out that the best way to remember something is to consciously try to *forget* it! Attention, as maid of all work, can be made to do anything and hence falls from its position of grace as a scientifically valuable concept. Thus the narrower definition of attention as "clearness," sponsored by Professor Titchener. According to this view, whatever is attended to, or in the focus of attention, is "clear," the rest being more or less unclear. It will be impossible here to explain the details of this theory as it concerns *attributive* and *cognitive* clearness and a number of other important distinctions. We shall consider only one type of problem to which the clearness theory has been applied.

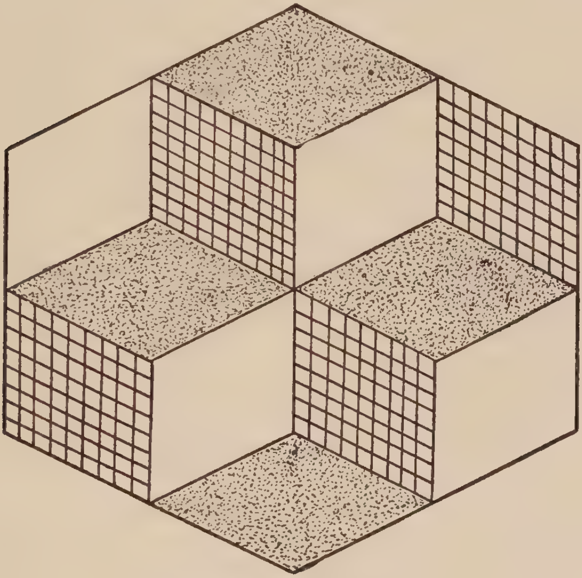


FIGURE 24

If we gaze intently at one of the outermost bands of the rotating Masson disc (Figure 23), part of the time a gray ring will be seen, part of the time it disappears. According to the theory we are considering, these fluctuations in the ring are changes in clearness. Likewise in the falling cubes (Figure 24) when the small, inner square is toward us and the outside square recedes from us, one part is said to be clear while the other is not; in the change which occurs when the rôles are reversed, we are told that the clearness has changed. But actual observation shows that what is seen during these fluctuations may be clear all the time. Something else is seen, however. *Something appears which was not present before and something disappears which was present before.* To say that merely a change in clearness has occurred implies that the object previously seen is still present, having changed only in clearness. But when the ring disappears into the background we do not see an unclear ring but a homogeneous field in which the ring no longer is present in any sense. To assume that the ring or the convex appearance of the small inner square still remains in perception when the change has occurred (with the statement that they are at a lower level of clearness) goes contrary to what is observed, perpetuates the constancy hypothesis and commits what is known as the "stimulus error" in psychology. The assumption is that when the change in clearness occurs the previously seen object remains because it may be there physically (the constancy hypothesis), confuses what is not seen with what is *known* under other conditions to be present—the stimulus error. We shall therefore find little use for this theory.

The Behavioristic Theory of Perception. At first sight it looks as if we delight in paradoxes when we speak of the behavioristic theory of perception because the members of this school have, in a sense, discarded the subject of perception from their repertory of scientific interests. Nevertheless there is a behavioristic theory of perception even though the members of this group do not agree upon the finer points involved in this question. Since Dr. John B. Watson in his book, *Psychology from the Standpoint of a Behaviorist*, 1924, treats of the facts of perception without using the term perception, we are able to construct what is probably the behaviorists' position.

To the behaviorist my perception of red, my feeling of fear, the taste of bitter are but the *response* of a special set of bodily organs or glands. Perceptions merely consist in the response of sense organs to stimuli of definite wave-length, mass or some other physical dimension. The *experience* of redness, warmth, strain, and the like do not enter into the theory; only the response of the retina, temperature spots and muscle spindles really matter. To such bodily changes perceptions reduce.

That there is a close correlation between what goes on in the sense organs and what is perceived, no one can doubt. But often a complete knowledge of what goes on in the end-organs fails to reveal the essential fact of the perception as such. Thus it is well known that the apparent size of objects does not strictly follow the law of visual angle which applies to the size of the image on the retina. Central factors make the simple physical law break down. In other words *what is seen* may have as much to do with the contents of perception as the conditions external to the actual perception. It may thus happen that the image of a coin on the retina is elliptical while the coin appears as round. Here the retinal response does not give us a true picture of the perceptual process. In order to account for all of the peculiarities in perceiving, an entirely different set of physiological processes located in an entirely different part of the optic sector may be required. In rejecting terms which have a mentalistic connotation the behaviorist often throws out interesting and important facts as well. Not to side-step but to meet facts squarely is the scientist's job. The simple expedient of reducing perception to the responses of end-organs, muscles, and glands fails for the finer aspects of the problem. We shall have to bring into our theory central brain processes which underlie every perception. The behaviorists have stressed the activities in the peripheral portions of the organism, omitting the most direct and important determinants of perception.

A Field Theory of Perception. A theory of perception must, in a sense, be more than a theory of perception. At least it must go beyond describing in more general terms what appears in a given case of perception. For we pointed out in the beginning that in perceiving the organism is *doing,* that perceiving is related to all the other activities of the organism, and hence our theory should enable us to better understand the other activities with which perception is intimately related. Perception leads over, on the one hand, to action of the overt, behavioral type, and, on the other hand, to the covert activities of memory, thinking and imagination. Our theory must therefore apply to the motor centers, the higher "association areas," and the activity of the brain as a whole, as well as the behavioral groups which these control. In other words, our theory of perception must be such in principle that it applies not only to the content of perception but also to the bodily activities issuing therefrom.

Instead of beginning, as so many psychologists have done, with simple sensory units out of which perceptions are supposed to be composed, let us assume that the simplest presentational datum is a homogeneous field. Certainly we can imagine nothing simpler, and, indeed, experience always presents us with *continua* or extended wholes. A second step in complexity would consist in a *differentiation* (*not* addition) within this primitive field. Thus a sound arising against a background of stillness, a bit of color against a background of gray, a flash of pain against a neutral or kinæsthetic field would come as differentiations within a field and might be likened to the single sensations of the older psychologists, except that we find they are derived in a far different way from that given in their theories. Further complexity comes by way of further differentiations within the given field. *There is thus no question of addition of simples to explain complexes.* What appears as figured against a background or field depends both upon the properties of the stimulus as such and the total field in which it acts. *Mutatis mutandis* the same reasoning can be applied to the central brain processes underlying the perception. The simple sensations of the introspective psychologists emerge as end-products or focal points in a highly complex field. We then seek the explanation of complexes not in sensations and subsidiary processes but rather in the properties belonging to extended wholes as such. It will be seen at once that this does not give us an universal explanation of all the moot questions of perception; it does point the way toward further research and specific hypotheses for specific classes of facts. And if it fits in with our present knowledge of neural functioning we have a theory capable of application to the activities related to perception.

There is much evidence to show that to perceptual fields there correspond physiologically extended processes which also have *field properties.* The old notion that point-like stimuli act upon point-like end-organs which send impulses along isolated nerve fibres to specific spots in the brain must be given up in the light of Lashley's work on the brain of the rat, Coghill's studies on the comparative anatomy of the nervous system and a host of other recent contributions to our knowledge of neurology. The well-established fact that the nerve impulse is, in last analysis, an electrical phenomenon forces us to regard the activities within the nervous system as dynamic and fluid in nature. To give the concrete evidence supporting the view advanced here that the nervous system is a highly interdependent, fluid mechanism in which each part influences every other and is in turn influenced by every other would require a separate section in itself. We pause only to point out that the facts of color and brightness contrast, the law of successive and simultaneous induction of colors, the perception of chords, melodies, spatial patterns in vision and on the skin, the fusions of taste and smell, all require a field theory of perception with underlying field processes in the nervous system.

We stated that an adequate theory of perception should apply to the motor and thought activities of the organism also. Our field theory of central physiological processes holds for the motor areas of the cortex as well as the sensory areas. Whether central brain processes issue in perceiving, action, or thinking they must, from the point of view of a general theory, be fundamentally similar. Just as the earliest and simplest perceptions are gross, so the earliest movements of the individual, phylogenetically, as well as ontogenetically, are ''mass actions'' or activities involving many muscle groups. The finer, specific responses to the details of the environment come with maturation and learning. Complex activities here, as in perception, develop as differentiations within the relatively simple and gross responses. The processes in the central nervous system, whether concerned with one type of activity or another, we assume to be due to an interaction of forces having the properties of extended wholes. While this theory implies that the brain functions as a whole (Lashley's law of ''mass action'') nevertheless it allows for more or less restricted fields within the total. Restricted fields derive their properties from the whole, and their parts, in turn, from the sub-wholes in which they appear. We are therefore able, by a process of differentiation, to account for whatever functional independence particular, specific responses exhibit. We have thus accounted for the facts which earlier psychologists have stressed without the assumptions we found so unsatisfactory in their theories.

We are now ready to return to more psychological questions involving the relationship of perception to other organic activities. We shall see how our view of total or mass action applies to and unites perception and behavior, recognition, and thinking, to mention only a few.

Perception and Behavior. Rubin (*Visuell wahrgenommene Figuren*, 1921) found that when he asked observers to reproduce drawings which they had seen they were better able to draw them when, in looking at the original, they grasped it as a whole.

If they regarded the contours of the figures to be reproduced, drawing was much more difficult. This finding was further substantiated by exposing the figures in a tachistoscope for two seconds: if the observers fixated the contours they were unable to reproduce since they could not perceive enough of the figures to begin a reproduction. If the surface of the figures was regarded, reproduction improved. It will be noticed that here success or failure in drawing, the kind of drawing made, depended, in large measure, upon *the mode of perceiving.* The conditions of perception are here antecedent to the actual drawing movements. We may generalize this fact by formulating a law which seems to have a wide range of validity: *how we react to a situation will be a function of how and what we perceive.*

Illustrations to substantiate this law further are to be found even in animal and abnormal psychology. In training apes to choose colored objects, Köhler found the animals could not distinguish between near-lying orange hues, *i.e.,* oranges containing nearly equal amounts of red and yellow. He then changed his procedure by presenting the apes with oranges of greatly different hue, one containing far more red than the other which was very yellowish. The apes then learned very readily to discriminate between them, always picking the correct one. Köhler then gradually decreased the amount of red until the two hues differed only slightly and the apes were now able to discriminate colors which before they had failed to do. Köhler had taken advantage of the fact that the ape could see or react to gross differences which were made finer and finer until the discrimination of small differences was established. This is a beautiful illustration of our field theory, for it is clear that it is much easier to react to two different fields or large differences within a given field than to the minor nuances within a field which approach homogeneity. We do not expect a differential response from an undifferentiated field. We have here a fruitful basis for the psychology of learning.

Gelb and Goldstein, two German investigators, working with subjects who had received brain injuries during the war, have provided us with a wealth of material from which we take but one or two illustrations. One patient could read or copy figures and letters only if he traced their outline. An injury to the occipital lobes prevented him from seeing a whole figure or word at once, much less complex objects. By means of eye- or head-movements he would construct the whole figure. When the word *Lazarette,* which he could finally make out, was cross-marked by lines through each letter, he failed, even with eye- and head-movements to identify the word! The cross-lines made the word so complex that to grasp it as a whole with the simple means at his command was impossible. His visual defect reduced him to the simplest perceptions and responses, for he was unable to recognize and handle complex objects beyond his simple perceptual capacities.

Perception and Recognition. We have already indicated that recognition depends upon the integrity of the mechanisms of perception. Rubin has reported (*op. cit.*) rather striking examples of this. If, in so-called ambiguous drawings, a field is seen as figure in one exposure, and as ground in a later exposure, the observer fails to recognize the fact that he is seeing the same drawing again. That this is not due to a mere change in the clearness of the figure is shown in the report of what the observer sees in the two cases: a field seen as figure is an entirely different object from the same field seen as ground. So radically have all of its properties changed that to say that merely the clearness dimension is different gives the impression that the same object is seen only at a different level of clearness. We may formulate the law that *recognition depends upon the preservation of figural properties, i.e.,* properties which make a field emerge or stand out from its surroundings. Human faces tend to do this for very obvious reasons and hence our ability to recognize persons after long years of absence. Since contours, outlines and forms are constitutive of figures, if these are preserved, the figural properties remain. Thus we recognize a painting under illumination which may change the color-value of every spot in it, if the contours are preserved.

Perception and Thinking. It will not be difficult to convince the reader that thinking, too, is a product of field forces and is closely related to perceiving. Often our solution to a difficult problem comes after we literally "see" some aspect or detail which modifies the whole in the right manner for the answer. Gelb and Goldstein's patient, referred to above, could not count from 7 to 10 without beginning at 1. Nor could he *see* that 2×6 and 3×4 had anything in common. He could not *see* that two symmetrical figures were similar. His power of inference, supplying missing terms in a proportion, everything requiring spatio-temporal frames was gone. These writers sum up his defects in this way: "Wherever a simultaneous survey of a membered structure is necessary in thought, this patient fails, just as he failed in his perceptual problems." We see, then, how intimately the processes of perception and thinking and ideation are related.

Perception and Feeling. It is almost trite to say that we react to every object, person and event not only with a motor response and more or less consciously, but also with an emotional reaction. Our principle that the organism responds as a whole to stimuli requires that the emotional aspect be included. We all know how certain people and situations "stir" us up in characteristic ways, pleasantly or unpleasantly, until the mere sight or sound of the stimulus in question becomes charged with feeling. Recognizing this fact common speech has invented such terms as "sullen" or "threatening" skies, "angry seas," a heart of "steel," and man of "iron." On the other hand, an angry or joyful mood may color every object we perceive. Our mood at a concert may accentuate the harshness of the brass or the sweetness of the violins. Certainly in the world of the feelings and emotions dynamic forces are at play and the entire organism is involved. Again we find that the various activities of the individual form an harmonious whole. When they do not, as in cases of dissociation and the like, the help of the psychiatrist must be sought to integrate the personality once more.

Conclusions. We have seen that perceptual, motor, ideational, and emotional processes cannot be separated either in theory or in fact. This essential fact of organic activity we have tried to do justice to in our field theory of perception. The actual laws governing the distribution, redistribution and final equilibrium of field forces, both in the end-organs and sensory-motor sectors, still remain to be worked out for special classes of problems. The recognition of the reality and importance of these problems and their explanation at an early stage of our psychological journey will do much to clarify our thinking as we proceed. If it is asked: "Then what is the use of knowing those other theories which we have found unsatisfactory?", we should remember that all theories have some modicum of truth in them and if we would know the history of our present state of knowledge and appreciate what is being done today, there is no better way than to review what the leaders in psychology have said and thought.

GLOSSARY

ACCRUING, arising as a consequent or result.

ASSOCIATION AREAS, areas of the cortex supposed to organize impulses arising in other areas.

ATTENSITY, the so-called clearness of particular mental processes.

ATTRIBUTIVE CLEARNESS, the clearness of a sensory process.

CENTRAL FACTORS, conditions in the brain.

COGNITIVE CLEARNESS, the clearness of a meaning.

CONSTANCY HYPOTHESIS, the assumption that there is a one to one correspondence between the action of a stimulus and some aspect of the resulting mental process.

COVERT, hidden; pertaining to behavior unobservable by another person.

END-ORGANS, sense organs.

EXISTENTIAL, given; present to sense; found already existing in experience; not derived by inference, interpretation, or association. According to certain theories sensations are supposed to be existential while meanings are derived through processes of association.

EXTENSITY, property of extendedness characteristic of many mental processes, for example the visual sensation of whiteness of a sheet of paper possesses extendedness; spatiality.

FIELD PROPERTIES, properties pertaining to a dynamic whole; properties derived from dynamic relationship, having no meaning without reference to the whole; examples: tension, potential, balance, organization, symmetry.

FUSIONS OF TASTE AND SMELL, example: ordinary "tastes," like that of lemonade, are part smell and part taste. The lemon aspect is an odor and the sourness is a taste.

INDUCTION OF COLORS, example: red light casts green shadows. The green in the shadows is "induced" by the surrounding red light; this is an example of induced contrast.

INTENSITY, property of mental processes such as intensity (loudness) of a tone, "heaviness" of a pressure against the skin, intensity of a pain, etc.

MEANING, the object-character or situation-character of experience; when an object or situation is perceived as a whole it has meaning, a quality or characteristic to be *grasped* as a whole, a quality or character over and above the parts. Thus, a certain thing perceived as a whole means *orange;* the parts—color, size, shape—taken alone do not mean orange.

MENTALISTIC, pertaining to mental processes; pertaining to an entity, mind.

MOTOR AREAS, areas having to do with control of muscular co-ordination.

MUSCLE SPINDLES, sense organs in the muscles.

Mutatis Mutandis, "the necessary changes being made."

NEUROLOGY, science of the nervous system.

OCCIPITAL LOBES, regions of the cortex at the back of the brain; contain the so-called visual area.

OPTIC SECTOR, nerve apparatus used in vision.

OVERT, open to view.

PHOTOCHEMICAL, chemical processes induced by the action of light.

Presentational Datum, item or fact of experience.

Protensity, the duration of particular mental processes.

Psychological Existence, that kind of existence in which the psychologist, as such, is interested; existential; not involving meaning.

Quality, the characteristic that differentiates one mental process from another, such as the hues of colors, bitterness or sourness of a taste, warmth or coldness of temperature experiences, etc.

Sensory Areas, areas first reached by nerve impulses entering the cortex from the sense organs of the body.

Sensory-motor Sector, a sector taking in both a sensory and a motor area.

Stimulus Error, confusing an object or a meaning with the sensory processes and imagery involved in perceiving that object or meaning; describing an experience in terms of the stimulus; the bias of being objectively minded.

Temperature Spots, spots on the skin from which experiences of warmth and cold can be elicited. The skin is not uniformly sensitive to stimulation when explored with a pointed object.

Univocally, always the same way; always of the same meaning.

Visual Angle, angle with apex in the eye and determined both by the diameter of the object seen and its distance from the retina; portion of the visual field subtended by a given object.

READING TWENTY-ONE [1]

AN ANALYSIS OF THE VISUAL PERCEPTION OF MOVEMENT [2]

Editor's Note

Gestalt or configurational psychology traces its immediate origin to experiments on apparent movement in 1912 by Professor Max Wertheimer. Apparent movement is an illusion that every one has seen in moving pictures and in moving electrical signs. In the laboratory it can be produced with two small slits cut in a cardboard screen, and a device to expose light from a source behind the screen first through one of these slits and then through the other. If the intensity of the light, the time interval of each exposure, the time interval between exposures and the distance between the slits are properly controlled, neither exposure is seen, but instead, a flash of light moving from the position of the first slit to the position of the second. There is a curious fact about the source of this illusion. The retina is stimulated in one region corresponding to one of the slits, then stimulated in another region corresponding to the other slit, but there is no stimulation of the retina in the region corresponding to the space between the two slits. However, the light is seen moving across the space between the two slits. Now if the light is exposed only through one slit, a stationary flash is seen. If the time interval between exposures is too long, first one band of light is seen and then the other, and if the time interval is too short, both bands are seen simultaneously. In neither of these two cases is there any apparent movement.

But this is not all. In case the conditions are just right for apparent movement, the movement is not seen until the *second* exposure is given. Moreover, if the light exposed through the second slit is stronger than the light exposed through the first, movement can be seen in the reverse direction from that in which the exposures are given. Apparent movement is obviously, therefore, a product of "field forces," that is, mutual adjustment of *two disturbances in the brain.* Before anything is seen, a differential of potentials must be set up, and the resulting movement depends upon a shift of energy from the higher to the lower of these two potentials. The exact details of this dynamic process are not known but their configurational character seems beyond doubt.

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[1] Parallels *The Science of Psychology,* Chapter XIII.

[2] By Harry R. DeSilva, University of Kansas. From the *British Journal of Psychology,* Vol. 19, Part 3, 1929, 269-273, 275-291. By permission.

This experiment was carried out in the Psychological Laboratory of the University of Cambridge, Cambridge, England, with the co-operation of Professor F. C. Bartlett.

The Author's theoretical discussion has been omitted. It should be mentioned, however, that he has drawn a parallel between theories advanced by the *Gestalt* Psychologists and Head's theory of dynamic "schemata," a theory which has not received the attention that its usefulness justifies.

It is claimed by the *Gestalt* psychologists that under the right conditions, apparent movement cannot be distinguished from the perception of the real movement produced by an actually moving object, and that both kinds of movement are to be explained by the same hypothesis. While the microscopically small, sensitive structures of the retina, are packed closely together, they are distinct, separate units, so that theoretically, the minute spaces between them give rise to the same problem as the space involved in the situation described for apparent movement. In the case of real movement, *continuous* movement is actually seen, but the stimulation is *discontinuous.*

It is not easy, however, to control the conditions so that apparent and real movement look exactly alike. Professor DeSilva has investigated this particular problem and his results are contained in the following article. His experiments reveal many of the difficulties which beset the psychologist when he wishes to make careful laboratory studies of observational behavior. In order to guarantee that experiences not intended for investigation will not be produced during the experiment, to the distraction of the observer, external conditions must be rigidly controlled. It is quite as important to control "internal" conditions as well, which exhibit themselves in the observer's attitudes and biases. So far as possible these internal conditions are controlled by instructions given to the observer. Professor DeSilva refers to these internal conditions when he speaks of "central determinations." Certain properties of the perception are in part dependent upon "central determination," as for example the degree of differentiation and organization in the detail of the object seen. The term clearness used by Professor DeSilva may be interpreted to mean degree of differentiation, a property often defined as "clearness of attention." To illustrate differentiation, suppose a student is looking through a highpowered microscope at some bodily tissue, say from the liver. This tissue would possess only a small fraction of the detail, for him, that it would possess for an observer trained in histology.

Professor DeSilva has also attacked the problem, What are the exact descriptive characteristics of apparent movement? Wertheimer came to the conclusion that when this movement is reduced to its simplest terms it is not seen as the movement *of anything*, not even of a light, but just as pure movement. To this experience he gave the name *phi-phenomenon.* There has been some question about the verifiability of this conclusion. Professor DeSilva has shown that whether the movement is seen as pure movement, or as the movement of something, depends upon "central determination."

I. INTRODUCTION

Although numerous experiments have been performed upon the perception of apparent visual movement, the divergence of findings, to say nothing of the divergence of theories, demonstrates that it is very difficult to isolate, describe and agree upon essential phenomena, and easy to omit from systematic description data which may be of fundamental importance. For example, even the basic assumption that apparent movement is subjectively the same as real movement, and that therefore all varieties of motion perception, both real and apparent, must involve essentially the same mechanism as that which underlies apparent movement, is extremely questionable. Far too little thought and experimentation have been devoted to real movement.

The chief obstacle to progress in our knowledge of the processes underlying visual motion obviously lies in the inherent complexity of both the phenomenon of apparent movement and the phenomenon of real movement. On the psychological side the experiences involved in perceiving movement are far more intricate and variable than in almost any other complex momentary static visual act. When, therefore, an attempt is made to pick out and to analyse the determinants of the perception of movement, both real and apparent, difficulties of various sorts assail one. Among such difficulties the following are outstanding: (*a*) a suitable apparatus must be constructed for presenting separately and contemporaneously real and apparent movement under varying stimulus conditions; (*b*) how far will it be possible to push an analysis of the experience? (*c*) what are the influences of observational attitudes, and of antecedent habits of interpretation?

A. APPARATUS AND STIMULI

The apparatus figured on the following page is a modification of the Dodge tachistoscope,[3] and possesses two principal advantages over the original model: (1) there are four exposure fields instead of two; (2) real movement may be presented separately or contemporaneously with apparent movement.

The stimuli employed in this investigation were slits of light on a dark field representing the simplest conditions under which the impression of movement can be obtained. This particular type of tachistoscopic stimulus was chosen because in this way the pre-exposure, between-exposure and post-exposure fields were made identical (*i.e.*, black) and could therefore have no disturbing effect on the experience. Likewise, the introduction of real movement into the spatio-temporal gap between the successively appearing discrete phases of an apparent movement situation was rendered possible without altering the background. Still

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[3] R. Dodge, *Psychol. Bull.* IV, 1907, 10-13.

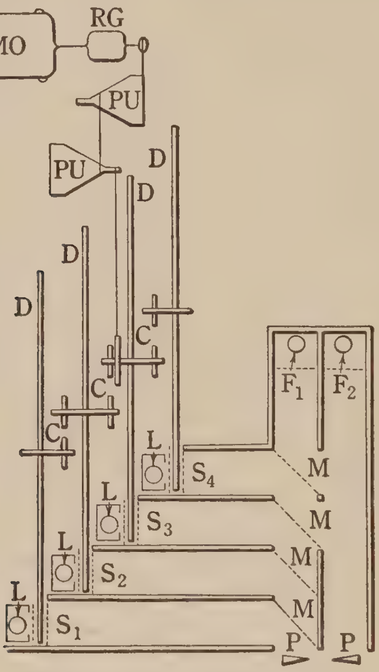
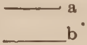


FIGURE 25. TACHISTOSCOPE

The drawing gives an overhead view of the tachistoscope. The body of the apparatus is made of wood and painted black within and without. MO: electric motor. RG: reducing gear. PU: adjustable grooved pulleys for regulating the velocity of the discs. D: circular cardboard discs 2 feet in diameter, with adjustable open sectors in the periphery. C: chain drive. L: Lamps with asbestos covering to confine the light to its proper path. S1, S2, S3, S4: stimuli consisting of slits in black paper held in position between clear glass and ground glass plates. The ground glass was placed behind the slit to prevent "glow" and shadows. M: mirrors of thin polished glass. F1, F2: small dim fixation points. P: prisms which serve to converge the two eyes.

another advantage of the black background was that the light intensity of the objects or slits could be varied without influencing the background.

Parallel lines were used rather than angular figures, or more complex figures, in order to simplify the excitatory processes involved. Unless stated otherwise, the stimulus used was two lines approximately parallel, thus . These lines were arranged slightly out of parallel so that real movement produced by a slit on the circumference of a large synchronously rotating disc might be introduced between the two members without altering the general appearance of things. Unless otherwise stated, the distance apart was 2.7 cm., width of lines .2 cm., length of lines 1.5 cm. A small dim dot of light in the middle of the field served as the fixation point. The visual angles for various distances apart of stimuli were as follows:

.5 cm.	equals	vis. angle	35′ 10″	about	40	cones
1.0	"	"	1° 8′ 52″	"	80	"
2.0	"	"	2° 20′ 18″	"	150	"
2.7	"	"	3° 10′ 11″	"	216	"

The optical distance from the stimulus to the observer's eye was 20 inches. The distance between the stimulus and the lamp was 3 inches. The illumination of the stimuli was from four 100 watt, 100 volt, gas-filled projection lamps. The transmission ratio of the ground glass plate behind the slits was *ca* (about) 50 per cent. The reflection ratio of the glass mirror was *ca* 40 per cent.

The observers included two persons who might be dubbed "trained observers," two advanced students in psychology, one outside student who had little or no previous experience in observing in psychological experiments, and the writer. All observers were not able to take part in each experiment, but the reports of at least four subjects were secured for each experiment.

The stimulus variables employed in the first part of the investigation are those which were first pointed out and stressed by Wertheimer[4] in his excellent paper. The reciprocal influences between these variables have been worked out by Korte[5] and Hillebrand,[6] but much work remains to be done to determine the absolute influence of such variables; for instance, are any particular variables more necessary to produce "full" or insistent movement than other variables? It is one of the purposes of this investigation to attempt to determine such dif-

[4] M. Wertheimer, *Zsch. f. Psychol.* LXI, 1912, 161-265.

[5] A. Korte, *Zsch. f. Psychol.* LXXII, 1915, 193-296.

[6] F. Hillebrand, *Zsch. f. Psychol.* LXXXIX, 1922, 209-272.

ferences, if any, between the amount of influence exercised by the most fundamental variables such as distance apart, light intensity, pause, and exposure time.

B. ANALYSIS

Wertheimer's comprehensive investigation of visual movement was perhaps the first study of apparent movement to emphasize the "going over" (*hinüber*) character of the phenomenon. This aspect he termed the *phi-phenomenon*. Wertheimer also made the distinction between static and dynamic elements, and stressed the latter as basic for apparent movement. Movement is a structured process unified in time. The locus of the experience must be sought in the whole, and the whole must be more than the whole presented at any single moment of the perception. The experience can only be taken as a totality in time. It cannot, he claimed, be broken up into simple static constituents as most of the previous investigators had tried to do. There is no doubt that the dynamic aspect of "movingness" or *phi-phenomenon* is a very important phenomenological characteristic of the experience of movement and that any analysis that fails to include some reference to the dynamic element is bound to prove inadequate. Nevertheless, although this spatio-temporal structure which may be termed "movingness" stands out in experience, need we, as *Gestalt* investigators would seem to recommend, posit *phi-phenomenon* as an ultimate, and shut our eyes to observable constituents such as the behaviour of the vehicle-of-movement, which also stands out in consciousness upon certain occasions. It is admittedly difficult, as Wertheimer has pointed out, to subject the experience of "movingness" or *phi-phenomenon* to analysis, particularly to analysis in terms of stable contents which are observable. But why is it difficult? A possible answer is, that under the usual conditions for presenting apparent movement three important factors restrict an application of analysis; namely: (1) the time intervals or pauses between the disappearance of one object and the appearance of the other object are very brief; (2) the distance through which the object moves or is supposed to move usually subtends a very small visual angle; (3) observers ordinarily possess no adequate criteria by which to rate the dynamic aspect of "movingness" on different occasions.

C. CENTRAL DETERMINATION

Apparent movement is dependent for its complete realization not only upon the stimuli but upon observational attitudes as well. The influence of psychic determinants upon apparent movement is a particularly baffling problem. One would imagine that if apparent movement is a correlative experience, qualitatively speaking, with clear real-movement-of-an-object, it should not be merely an experience of ''movingness'' or *phi-phenomenon*, but an experience of ''movement-of-an-object,'' because practically all real movement has a one-object reference. In cases such as those reported under the term ''pure-*phi-phenomenon*'' the ''of-an-object'' aspect of the experience is certainly at a different level of clearness from the ''of-an-object'' aspect of the experience such as is reported under the ''full movement'' or ordinary *phi-phenomenon*. Likewise, as the author has shown elsewhere,[7] this pure *phi* taken as a unique complete experience appears mainly when the report shows that the data from the ''of-an-object'' aspect are not clear in experience. Consequently, if the ''movingness'' aspect and the ''of-an-object'' aspect of an experience in one case have an entirely different ''clearness status'' from the ''movingness'' aspect and ''of-an-object'' aspect in another case, then the influence of clearness differences of the experience of movement under different occasions should not be regarded as irrelevant and neglected as has been done in former studies upon apparent movement.

II. EXPERIMENTAL

A. EXPERIMENTS CONCERNED WITH STIMULUS DETERMINANTS

In his 1912 investigation Wertheimer came to the conclusion from a few casual experiments that there was no constitutive difference between real and apparent movement,[8] and that the fundamental element in all movement phenomena was a ''going-over'' (*hinüber*) or ''movingness'' which he termed the *phi-phenomenon*. Most of the investigators who have followed Wertheimer have accepted this fundamental assumption that apparent movement is subjectively the same as real movement, and that therefore all motion perception, including real movement, must involve essentially the same mechanism as that which

[7] *Amer. J. of Psychol.* XXVII, 1926, 496.

[8] *Zsch. f. Psychol.* LXI, 1912, 169-174.

underlies apparent movement. Recently, Dimmick and Scahill made a direct attack upon this problem of the introspective differences between real and apparent movement and succeeded in finding certain differentiæ. In their conclusion, however, they fail to emphasize differentiæ between real and apparent movement which their observers reported, the experimenters made no attempt to vary the stimulus conditions, for example: they used only one exposure time, one pause (or velocity of real movement), and one distance between objects. As will be demonstrated in the three experiments which follow, the above-mentioned stimulus variables are extremely important when making a comparison between real and apparent movement.

First experiment. Distance apart

To measure the influence of the factor of distance apart when contrasting apparent movement with real movement. Three spatial intervals were employed: (*a*) long 2.7 cm.; (*b*) medium 1.2 cm.; (*c*) short .5 cm. Two stimuli were presented in quick succession. Sometimes a line came in when the first line disappeared, moved down, and disappeared when the second member appeared. At other times this real movement-of-a-line was omitted. The order of presentation of apparent movement and apparent-real movement was haphazard. Except where otherwise stated the exposure time was 50 sigma, pause 50 sigma, angular velocity of moving object *ca* 58° per sec.

Results.

1. When the distance between the members was (*a*) long, all observers were able to distinguish consistently presentations of apparent movement from presentations of real movement.
2. When the distance between the members was (*b*) medium length, observers confused apparent movement with real movement from 10 per cent. to 40 per cent. of the time.
3. When the distance between the members was the short length (*c*), observers were totally incapable of distinguishing between real and apparent movement. Observers often reported "apparent movement" when real movement was present. Likewise, observers often reported "real movement" when apparent movement was present.
4. Reports upon qualitative differences between real and apparent movement with different distances indicated that in

the case of the long distance (*a*), observers were nearly always aware of the fact that "the line did not move down as a line the whole distance." In the case of apparent movement observers sometimes reported that there was a "partial movement" of one or both of the lines. At other times they "felt" or "inferred" that the line "dropped down," even though little experiential data were present to confirm this inference. In the case of the middle distance (*b*), observers were less able to see clearly what happened in the space between the members. The experiential data were more ambiguous, and apparent movement was confused more often with real movement. In the case of the short distance (*c*) the experiential data of the events from the space between the lines were still less obvious. Except when the velocity of the real movement was very slow, the apparent movement and the real movement were almost always indistinguishable.

5. Observers varied considerably among themselves in the accuracy of their judgments upon the "realness" or "illusoriness" of their experiences. One observer was much more accurate than the others. This accurate observer also seemed rather more confident of his judgments than was the case with other observers. The descriptive adequacy of the accurate observer, as indicated by his ability to coin phrases to "catch up" his elusive experiences, was also exceptional.

6. Two observers reported that the "carry over" of the movement in apparent movement situations suffered considerably after seeing and comparing real movement with apparent movement. These two observers became much more critical of the experiences from the apparent movement situation and reported fewer cases of "carry over" of the movement when there was no real movement.

Second experiment. Velocity and pause

To determine the differences, if any, between real and apparent movement under varying velocities of real movement and pauses of apparent movement. The first part of the experiment (A) consisted in obtaining reports upon real movement at different velocities over the long distance, 2.7 cm. The second part of the experiment (B) consisted in comparing a perception of apparent movement with a perception of apparent movement in which the spatio-temporal gap was filled with a real moving line.

Part A.

As the angular velocity of the real movement was increased from a very slow speed, 3° 10′ per sec., to a very high speed, *ca* 116° per sec., the following changes were observed.

1. At first a bar of light with distinct contours was seen moving slowly across the field. Angular velocity 3° 10′.

2. The bar became slightly "blurry" and less clear in outline. It could be seen to move across the field as a "blurry" line the whole distance. Angular velocity 10° to 14°.

3. A bar of light with a luminous "tail" or "sheet" moved or "floated" across the field. The bar of light was rather vague. Angular velocity 14° to 21°.

4. A "sheet of light unfolds from the top" and sometimes "folds up again at the bottom." At other times the "sheet of light" vanishes from view as a "sheet." Angular velocity 21° to 58°.

5. A "slightly vibrating sheet of light" covers the whole field, giving the impression of direction downward. Angular velocity 58° to 116°.

6. A "stationary sheet of light" with little or no indication of the direction from which it came. Angular velocity above 116°.

Part B.

1. When the velocity of the real movement was in the neighbourhood of 3° 10′ per sec., observers were always able to distinguish real movement from apparent movement, no matter what the total distance travelled was.

2. When the angular velocity was 10° per sec., observers found it much more difficult to distinguish real movement from apparent movement, especially when the distances travelled were .5 cm. and 1.2 cm. When the total distance of 2.7 cm. was traversed, the observers were usually able to distinguish the real from the apparent movement.

3. When the angular velocity was 21° per sec., observers were unable to judge accurately between real movement and apparent movement, when the small (*c*) and medium (*b*) distances were employed. It was difficult but not impossible to distinguish real from apparent movement when the long distance (*a*) was used.

4. When the angular velocity was greater than 21° per sec., observers found it rather more easy to judge accurately between the real movement and the apparent movement than when the angular velocity was between *ca* 10° and 21°.

Third experiment. Exposition time and light-intensity

To determine the influence of exposure time and light intensity of the lines on the perception of movement. Four exposure times were employed: (*a*) 10 sigma; (*b*) 50 sigma; (*c*) 200 sigma; (*d*) 400 sigma. The pause was 50 sigma, and the distance apart 2.7 cm. unless specified otherwise.

Results.

1. When the exposure time of the lines was made very short (*a*), observers reported that the lines were "dim," "jumpy," "shaky," "jerky." With the intensities of light used in the experiment this intramembral movement had little direct favourable influence on the "carry over" of the movement.
2. When the lines were exposed for long intervals, (*c*) and (*d*), this intramembral movement was absent.
3. When the lines were made as bright as possible by taking out the ground glass plates, the long exposures, (*c*) and (*d*), caused the lights to appear "flashy" and to spread and swell beyond their usual distinct boundary. In such cases the space between the lines often appeared "luminous" and the "carry over" effect of the movement seemed to be more compulsory.
4. When the lines were made quite dim by reducing the illumination (resistances) the intramembral movement was increased but the "going-over-ness" was not improved.
5. When the light intensity of the stimulus for real movement was increased considerably by removing the ground glass plates, observers found themselves able to distinguish between real movement and apparent movement rather better than usual. In the case of real movement there was "something" in the space between the lines, whereas in the case of apparent movement this "something" was absent.
6. When the light intensity of the stimulus for real movement was decreased considerably by a resistance, the observers were less able to distinguish between real movement and apparent movement than under the usual conditions of illumination.
7. Lengthening the exposure time of the first light has a more

decided effect on lessening the "carry over" of the movement than lengthening the exposure time of the second member has.

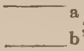
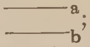
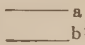
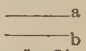
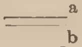
8. Varying the exposure time of either object had little effect on the accuracy of observers in distinguishing between real and apparent movement.

B. EXPERIMENTS CONCERNED MAINLY WITH CENTRAL DETERMINANTS

Problem.

To study some of the effects of central determinants such as psychological clearness, expectancy, sets, etc., on the perception of apparent movement. Several experiments are included under this head and will be dealt with separately.

Fourth experiment

Real movement and apparent movement were combined in various ways. The end members producing the apparent movement were always at the full distance apart, 2.7 cm. The real movement, on the other hand, sometimes extended over (1) the full distance ; sometimes it extended over (2) the first half of the total distance ; sometimes it extended over (3) the last half of the total distance ; sometimes it extended over (4) the middle half of the total distance ; and sometimes over (5) the middle quarter of the total distance . The exposure time was 50 sigma, pause 50 sigma, angular velocity of real movement 58° per sec.

Results.

1. Observers were found to vary considerably in their ability to distinguish between the various combinations of real and apparent movement. Two observers were able to distinguish and describe accurately the position and the space covered by the real movement in every case but number (4). All observers reported accurately upon number (5) after seeing it no more than twice. Numbers (2) and (4) were most often confused with number (1).

2. After looking at the partial-real movements repeatedly, all observers were eventually able to report accurately upon them.

3. When the velocity of the real movement was reduced to 10° per sec. there were rarely any failures at distinguishing apparent movement from the various kinds of real movement.

Discussion.

This experiment demonstrates the paucity of the observer's bare experiential data of the events which take place in the spatial interval involved in the apparent movement situation. The results also represent an excellent example of the "filling in" activity of the organism when it is confronted by incomplete, obscure data.

Fifth experiment.

In this experiment the observers were told that they would be given a long series of presentations and that they were to observe carefully so as to be able to report upon their experiences at any time. Apparent movement only was used. The usual stimulus conditions were employed with exposure times of 50 sigma, and pause of 50 sigma. After movement had been presented in one direction for a number of times, the direction of presentation was suddenly reversed and the observer asked to report.

Results.

1. At the beginning of the experiment for at least the first three reversals all observers reported that the movement proceeded in the expected direction, which was, of course, contrary to the order of appearance of the stimulus. It was often reported that the last member of the series seemed "a bit queer," "was slower," "was jerkier," but definitely that the movement proceeded in the usual direction. After three or more series all observers commenced to see the movement reversed, and after once seeing the final presentation reversed they always saw it reversed subsequently. The nature of the experiment was afterwards explained to the observers and they corroborated the facts in their written reports, that is to say, they reaffirmed that they really saw the movement proceed in the same direction all through the series.
2. One observer continued to see the movement go in the same direction for ten series. Finally, the experimenter told her what was happening and she immediately saw the move-

ment "carry over" in the opposite direction in the last presentation of the next series.

3. One observer reported that the movement seemed to "carry over" less well than usual, during the reversal experiment.

4. No difference was noted in the "carry over" of the standard stimulus when it reappeared immediately after a reverse stimulus.

Discussion.

The results indicate that changes in the stimulus which appear suddenly and unexpectedly may not immediately effect a gross change of direction in the experience, but instead may only result in a loss of "clearness." After the stimulus has been observed repeatedly, the perception may "clear up" and be correlated more exactly with the stimulus.

Sixth experiment

The following stimuli, represented by the lines in the diagrams, were given in pairs in various orders.

	—1		—1				—2				—2		—1		—2
(*a*)	—2	(*b*)		(*c*)	—1	(*d*)	—1	(*e*)	—2	(*f*)		(*g*)	—2	(*h*)	—1
			—2		—2				—1		—1		—1		—2

The usual exposure time, 50 sigma, and pause, 50 sigma, were employed. The temporal interval between one presentation and the next presentation was approximately 2 seconds. Observers were asked to give their expectations, if any.

Results of fifth and sixth experiments.

1. Observers reported that whenever they expected or were "set" for the second presentation to appear in a certain place and go a certain way, and it went another way, they were unable to apprehend it so clearly as usual.

2. At the beginning of the experiment observers usually found themselves expecting the movement to go in a certain direction. After they had erred a sufficiently large number of times this expectation often disappeared, so that eventually observers experienced no particular conscious set for any particular direction.

3. The observers were clearly more confused, bewildered and inaccurate than usual under the conditions of the experiment,

although after they had become thoroughly acquainted with all the possibilities and limits of the experiment a good deal of this confusion and inaccuracy disappeared.

4. In the case of the double movement arrangement (*g*) and (*h*), no observer was able to report accurately upon the experience until after he had observed it three or four times. Three observers reported only two lines when either arrangement was seen for the first time.

5. It was noted during the first few times that observers saw this equivocal figure that the apparent direction of the movement was nearly always the same as the direction of the preceding movement presentation. When, however, the observers had become acquainted with the figures, the direction of the preceding movement presentation exercised no influence, *i.e.*, the movement in either direction might dominate in the equivocal figure, or in some cases the movement in the opposite directions might be equally impressive, producing a "splitting movement."

Discussion.

This experiment illustrates the common habit among observers of "cognizing" or "getting acquainted with" or "expecting" the phenomenon before it appears. First reports on unexpected apparent movement situations indicate that the visual data were unclear and inadequate. After repetition, the visual data became more adequate. The observers experienced greater confidence and were able to report definitely and with a fair degree of accuracy. In general, the more complicated and unexpected the figure the greater the difficulty in reporting.

Seventh experiment

In this experiment the temporal interval between the second member of an apparent left-right movement | → | and the second member of a subsequent right-left apparent movement | ← |, was varied between 0 sigma and 200 sigma. The second member by the left-right movement served as the first member of the right-left movement. The exposure times of the individual lines was 50 sigma, and the pause was 50 sigma, unless otherwise noted.

Results.

1. As the interval between presentations is lengthened or shortened, the apparent pause (*Zwischenzeit*) between the mem-

bers of each presentation seemed to suffer an increase or decrease, that is to say, when the time interval between the presentations is shortened to 30 sigma, the apparent duration of the 50 sigma pause between the individual members of the two presentations seemed to be much shorter than when the time interval between presentations was 200 sigma.

2. When the time interval between presentations is increased to 100 sigma or 150 sigma the three movements right |→|, left |←|, right |→|, are no longer seen. Instead the observer sees two "right" movements with only a sort of "flick" or "blink" between. When the interval between presentations is increased to 200 sigma this "flick" disappears and the observer apprehends two "right" movements with no conscious connection between them.

3. Four observers reported that the first movement presentation was "slower" and "carried over" better than the second presentation, in spite of the fact that the pause between the two presentations was always identical. The oldest and most experienced observer never got a good "carry over" of the movement from the last member of the first presentation to the first member of the second presentation.

4. When the time interval between the two presentations is zero; *i.e.*, no interval between the disappearance of the second member of the first presentation and the first member of the second presentation, observers get a very slight indication of movement from the first presentation, and then merely a "wobble" from the second presentation. The appearance of the second presentation immediately after the first presentation does definitely upset any "carry over" of movement that might have resulted from the first presentation given alone.

Discussion.

This experiment indicates the importance of an adequate lapse of time between the disappearance of one movement presentation and the appearance of the next presentation, in order that either presentation may have time to "clear up" in consciousness. When the pause between the disappearance of the second line and the appearance of the third line was lengthened the second movement phase was obliterated, and movement phase one and movement phase three appeared separately as two structures separated in time. When, however, the temporal interval of the second movement phase was made much shorter than the pauses of movement phases one and three, the resulting increase in the apparent velocity of movement phase two effected an increase in the apparent velocity of phases one and three.

Eighth experiment

In this experiment the two horizontal parallel lines were presented alternately to an observer repeatedly. The pause between lines was varied from 100 sigma to 33 sigma. The time of exposure of the lines was always equal to the dark interval or pause between the lines.

Results.

1. With very long pauses, *e.g.,* 500 sigma to 100 sigma, there was no insistent experiential basis for "carry over," nor was there any inference of movement. One observer reported that he could "imagine that the beam of light moved behind the background and appeared below," but this experience he admitted was of a "highly inferential, non-compulsory type."
2. With shorter pauses, *ca* 200 sigma, a "moving succession" was reported: "see-saw motion," "something like the balance wheel of a watch," "bar of light hinged on a horizontal axis with the fixation point in its centre swinging up and down in an arc from position 1 to position 2," "bar moves vertically up and down."
3. With a pause of *ca* 50 sigma the reports were similar to those mentioned in the paragraph above.
4. With pauses below 50 sigma, the experience tended to approximate more to two stationary but swelling and contracting lights with little or no tendency for a "carry over" to take place.
5. Fixating points to the right, left, above, or below caused no noticeable change in the phenomenon when the pause was 50 sigma. Fixating the place where one of the lines appeared tended to diminish the "carry over" of the movement when it was good. Two observers reported that the "carry over" was improved when the eyes were allowed to relax so that the accommodation was for infinity.
6. Voluntary attempts to alter the "carry over" of the movement, *i.e.,* to improve or diminish it, were unsuccessful.
7. When the successive appearance of one line was suddenly omitted most observers reported an impression of "partial movement of the remaining line toward the place where the

other line should be,'' ''top light swelled out, downwards and sideways where light No. 2 should have been,'' ''carry over of the swinging motion of the remaining light for five or six beats,'' ''single line moved slightly downwards.'' This ''carry over'' of the swinging motion was very much better when the observer was taken wholly unawares and had no notion of what was coming. When an observer ''expected'' the disappearance of one line, the ''carry over'' was not nearly so clear.

8. The movement or ''carry over'' was reported as more insistent and compulsory with serial presentation than with discontinuous single presentations of movement in one direction.

9. At first certain observers expressed a lack of confidence and dissatisfaction in the adequacy of their reports. After continued observation and routinizing of the report a greater degree of confidence appeared.

Discussion.

The ''carry over'' of the movement is more insistent when the stimulus is repeated in quick succession than when it is given discontinuously. The ''carry over'' of the movement even when the second stimulus is lacking seems to indicate that the area of regard, which may or may not correspond with fixation, may be displaced automatically back and forth during the continuous up-down-up movement.

C. SUMMARY OF EXPERIMENTAL RESULTS

A survey of the protocols of this investigation shows that reports of observers centre around two fundamental aspects or attributes of the phenomenon of movement, namely, ''movingness'' and the vehicle-of-movement.

1. *''Movingness''*

Because of our everyday habits of thinking of psychological processes in terms of the conceptual ''objects'' of physics, we find it difficult to conceive of ''movingness'' apart from movement of a physical object. In substantivizing the activity designated by the verb ''to move'' as ''movingness,'' we seem at first sight to be dealing with an unnecessary bit of abstract fiction. Nevertheless, even the most casual observation of either real or apparent movement in its simplest form reveals an individualized and unique psychological experience of "movingness" or *phi-phenomenon.* A careful analysis of the nature of "movingness," as gleaned from the protocols of this investigation, reveals the following facts: (1) the attributes of quality and intensity which usually stand out in the perception of stable objects do not exercise as much influence in the experience of "movingness"; (2) the attributes of extensity and protensity are very intimately tied together in the experience of "movingness"; (3) on the stimulus side "movingness" is most directly comparable to the progressive spatio-temporal process which we speak of in physics as velocity. The immediate perception of "movingness" does not come into existence except when the angular velocity of the physical object lies within certain limits.[9]

As a psychological entity, therefore, "movingness" involves an integration of spatial change in a temporal structure, which is immediately present in consciousness, together with secondary attributes (described in terms of physics) of velocity and direction. Moreover, like nearly all visual psychological structures, it is anchored in space. "Movingness" varies constitutively with various velocities of the moving object, and with the effectiveness of central determination. The type of stimulus giving rise to "movingness" possesses an especially low threshold for attentive consciousness. Consequently, "movingness" clears up in consciousness even when the object which carries the movement is incompletely represented.

2. *Vehicle-of-movement*

Besides mere "movingness" there is in the case of movement with slow velocity an additional awareness of a "that" or object-which-moves.[10] This object-reference in the perception of movement varies considerably in "clearness status," and most reports upon apparent movement are inadequate chiefly in respect of the "clearness status" of the vehicle-of-movement. Due to the tenuous and transient nature of the excitatory processes involved in the perception of movement, the observable data from the vehicle-of-movement, especially during-its-passage, often possess low degrees of "clearness" in consciousness. The chief distinction between slow and fast real movement, and between real movement and apparent movement seems to lie in the relative ''clearness'' or obscurity of the vehicle-of-movement.

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[9] *Cf.* results of Exp. 2.

[10] Visual movement as defined by Dimmick is but duration and quality. Dimmick omitted this very essential constituent of vehicle-of-movement which is present in any "clear" perception of movement.

3. *Real movement*

The main stimulus variable of real movement is the velocity. As Exp. 2 indicates, when the velocity of movement is slow, both ''movingness'' and the vehicle-of-movement may possess a high degree of ''clearness.'' A telescoped report of this experience might read as follows: ''movement-of-an-object-across-the-field.''

When the velocity of the movement is high the ''movingness'' may still remain focal in consciousness, but the vehicle-of-movement is represented at a much lower level of clearness. A telescoped report of the experience would be likely to read like this: ''movement-across-the-field'' or else ''movement-of-something-across-the-field.'' With very high velocities all details of the vehicle-of-movement aspect may be absent. Of course, if the velocity is sufficiently high even ''movingness'' itself will disappear.

4. *Apparent movement*

As has been indicated in Exps. 1, 2 and 3 of this investigation, the chief stimulus variables of apparent movement are: (*a*) distance apart of objects; (*b*) pause; (*c*) exposure time; (*d*) light intensity.

(*a*) *Distance apart.* It is to be gathered from Exp. 1 that the success of the illusion of apparent movement as measured by the ability of the observer to distinguish between real movement and apparent movement was dependent primarily upon a short distance between the successively appearing objects. As the distance between the objects was lengthened the illusion of movement became progressively poorer.[11] A telescoped report of an average experience of apparent movement when the distance between the objects was small would read either: "object here—movingness—object there" or else "object here—object moved down—object there." Although reports upon vehicle-of-movement may appear, it can be definitely stated that in no case when the distance between the successively appearing objects was small did the vehicle-of-movement, or the events occurring in the space-between-the-lines, occur at a high level of clearness.

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[11] The perception of angular movement is complicated considerably owing to the variable distance apart of different portions of the two lines; *e.g.*, $a \angle b$. The movement near the axis of rotation may appear to "go over" because the gap is so small. On the other hand, the ends of the lines may not "carry over" so well. In other words, the illusion of movement may vary in degrees at different distances away from the centre of rotation, being most favourable near the axis and poorest near the periphery. The perceptual processes are accordingly highly complicated and such determinants as visual fixation and attention, which are so difficult to control, play an even more important part than usual.

(*b*) *Pause.* Exp. 2 demonstrated that the success of the illusion of movement, as measured by the ability of the observer to distinguish between real movement and apparent movement (when the distance is fairly small), depended chiefly upon the pause being kept between *ca* 25 sigma[12] and 200 sigma. Within these brief intervals of time the visual contents of the space-between-the-lines seldom had a chance to "clear up" in consciousness.

(*c*) *Exposure time.* (*d*) *Light intensity.* Varying the exposure time and light intensity of the end members of an apparent movement stimulus did not exercise any very striking influence on the experience as a whole, except that a pronounced increase of the light intensity of the end members seemed to favour the illusion of movement. The reason for this enhancement of the illusion of movement seemed to lie in the fact that a pronounced increase in the light intensity of the two members enlarged the actual dimensions of the objects themselves by a "glow," decreased the apparent distance between the objects, and filled the intervening space with a diffused light. All of these influences assisted in making the observer less able to distinguish the real from the apparent movement.

5. *Central determination*

The experiments upon central determinants illustrate the paucity of experiential data from which the perception of movement may be obtained. Consequently, under suitable conditions of background and light intensity the impression of real movement can be produced by eliminating certain stages of the real movement of an object. In fact, the whole series of events between the first appearance of the line-which-is-to-move, and the last appearance of the line-which-has-moved, may be eliminated, and still, under favourable circumstances of peripheral reception and central disposition, the impression of movement may arise.

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[12] It was not possible to run the discs at a rate higher than that which produced an angular velocity of *ca* 116° per sec. This velocity of real movement corresponded to a pause between members of the apparent movement stimulus of *ca* 25 sigma.

Let us picture a typical ''favourable'' situation. A person is an observer in a laboratory experiment in which there is a sanction, if not a compulsion, to give a report. This person, let us say, is presented with a stimulus involving two objects which appear successively and are separated by a short spatial interval. The data of the events which occur in the spatio-temporal gap are obscure in consciousness. When confronted by such a situation, what could be more natural than for our observer to apprehend the total, brief, obscure, experience as a ''meaningful'' event, which in this case, in view of his past experience with moving objects, would result in his filling the spatio-temporal gap with the conscious correlate of similar habitual experiences, namely, with movement. There were two objects, there was ''movingness,'' and therefore what could be more likely than to assume that the first object moved over to the second object? But some one will say, ''What of these persons, 'trained observers' perhaps, who cannot 'see' apparent movement?'' There are many persons who refuse to ''see'' apparent movement. In fact, observers of apparent movement might be divided into two types: ''those who see it'' and ''those who don't see it.'' Very well, let us take a ''trained observer'' who does not ''see'' the illusion. Let us put him in the laboratory situation and subject him to ''apparent movement'' under the optimal stimulus conditions, *i.e.*, short distance between stimuli, pause of 50 sigma, fairly bright lines, etc. What will our ''trained observer'' report? As Exp. 1 has shown, very probably he will report ''no movement,'' or ''vague inference of movement,'' ''line here—greyness—line there.'' Then without his knowledge let us introduce rapid real movement into the spatio-temporal gap. What then will be his report? Quite likely, as Exp. 1 has demonstrated, ''same phenomenon'' or ''no movement.'' With stimulus conditions for apparent movement we can get reports of ''real movement,'' and with stimulus conditions for real movement we can get reports of ''no movement'' or ''apparent movement.'' Detailed introspective reports of observers upon rapid real movement, or upon apparent movement which involves a short spatio-temporal gap, can never be wholly adequate and reliable.

GLOSSARY

Angular Velocity, rate of movement about a point or along an arc; rate of movement of a radius.

ca, about; approximately.

Clearness-status, degree of differentiation and organization of a configuration.

Dynamic Element, feature of experience that is constantly undergoing some sort of change.

Experiential Data, introspective data; psychologically described items of experience.

Integration of Spatial Change in a Temporal Structure, changed relations of parts taking place in time when both time and space factors determine the relationships between the parts.

Intramembral Movement, movement or changes within a moving object or field.

Phi-phenomenon, the experience of movement without the experience of anything moving; non-sensory awareness of movement.

Protocols, introspective reports.

Sigma, one thousandth of a second.

Spatio-temporal Structure, a complex mental process whose configuration or structure is organized both in space and time; the dynamic processes constituting the whole require both space and time for their relationships.

Static Element, feature of an experience that undergoes little or no change.

Substantize, to define or describe a thing as if it were a substance or entity; to objectify or define a thing as if it were an object when it is not.

Transmission Ratio, the proportion of light transmitted through a given medium, such as glass; different kinds of glass cut down the light to different amounts.

READING TWENTY-TWO[1]

THE EFFECTS OF DIRECT STIMULATION OF THE BLIND-SPOT[2]

Editor's Note

The Configurational Character of Sensory Processes.—To illustrate how apparent movement follows each of the eight organismic laws presented in the first Reading would be an easy matter, a task which the beginning student might now set up for himself. It is easy because a sufficient number of the conditions for obtaining the illusion can easily be recognized. But it is a different story when observational behavior is so limited in its character by restricting conditions that the process under observation seems to be unrelated to anything else observable at the time. Consider the conditions of behavior now so restricted that the experiences are of the simplest possible kind.

The complexity of stimulus-patterns has been reduced to single types of forces impinging upon the sense organs of the body. Light is allowed to affect the retina; sound waves are allowed to affect the ear; pressure is applied to the skin; a solution is applied to the tongue—all under the simplest possible conditions. A so-called sensation will be observed: A brightness or a color, a tone, a pressure or a pain, a taste quality. In their very simplicity they seem to stand alone, a whole without parts. Since they apparently stand alone, their independence seems complete save for the stimulus or external force, their only apparent cause (the constancy hypothesis). One is reminded of the dilemma in which the physicist would find himself if he were investigating isolated electrons. The conditions of observation would be so restricted that the electron's behavior could not be observed in relation to anything; the whole of which it is a part would not be evident. As a consequence the physicist would not know what the electron is doing; it would seem to be wandering around at random or in other words "going wherever it wanted to." Naturally this is merely a figure of speech, but it illustrates the fact that nothing scientific can be said about the behavior of anything when it is not studied in relation to some whole.

Take an illustration from biology. There are certain organic molecules, called genes, in the germ plasm. Formerly it was thought that a single gene was the determinant of a particular organ such as the eye, because destroying a gene prevented the eye from developing in the subsequent embryo. But the discovery was made recently that a whole pattern of genes is essential for the development of an eye, because the destruction of any one gene of the pattern prevents the eye from forming. The exact rôle played by the individual gene is still a question, except that it is necessary to complete the pattern.

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[1] Parallels *The Science of Psychology*, Chapter XIV.

[2] By Harry Helson, Bryn Mawr College. From the *American Journal of Psychology*, Vol. 41, No. 3, 1929, 345, 346, 347, 364-370, 372, 373, 374, 375-377, 380-382, 384-397. By permission.

These illustrations serve to guide the psychologist in his attitude toward "sensations." Because they can be controlled so readily by a single factor, namely, the external stimulus, is no sign that configurational principles do not apply to them.

In the first place they are not so simple as they seem. Look at any red. The trained observer can always see either some blue or some yellow in it; aside from redness it also has brightness and saturation, spatiality and duration—the so-called attributes of sensation. Of special importance, however, is the lack of qualitative purity; there is, psychologically, no absolutely pure red. Many observers insist that in the perception of red there is also a kinæsthesis or a pressure, which is not an extraneous factor. The situation is more obvious when tones are considered, as was pointed out in Reading Nineteen. It is doubtful if any one ever heard a psychologically pure tone. Those who do not see as well as hear a tone, *feel* it; the quality is round, smooth, harsh, raspy, or mellow, in other words *borrowed* from the sense departments of touch and kinæsthesis. Take pressure sensations. One can discover either visual imagery or kinæsthesis in his awareness of pressure. The simplest conceivable mental process, then, is qualitatively complex. Accordingly, external stimulation is not the only conditioning factor. Factors within the organism play a very important part in bringing the "sensation" into existence, and these factors are internal *field properties* for which the qualities "borrowed" from other sense departments are evidence. Perceiving simple sensations, therefore, is a function of the organism as a whole.

In the second place, it can be shown that qualities depend upon a ground. On a red ground a grey patch looks green and on a blue one it looks yellow. When sight is restored to a congenital blind person he cannot see instantly; vision must emerge from a tactual and kinæsthetic ground; and after he discovers the new phenomenon light, particular objects must emerge from a visual ground with the aid of touch and muscular movement. Presumably, in individuals who see, a similar but a slower process takes place early in infancy. These emerging qualities are complex enough to be "figured," or differentiated. Indeed, configuration within sensory qualities, not alone within the more complex patterns, such as geometrical designs, seems to be necessary for membership or structurization within a larger whole. A certain color may in part derive its quality, say yellowness, from a photochemical substance in the retina; a tone may in part depend upon a particular rate of vibration of nerve endings in the ear, but so long as these particular conditions at the sense organ are studied without reference to the remaining conditions, all of which are equally important, the belief will remain popular that sensory processes do not fit into an organismic system.

An excellent illustration of this bias can be found in the long prevailing notion that the optic disk is a *blind-spot.* The optic disk is the region or place where nerve fibers from the retina congregate and form the optic nerve. Investigators have long since known that one can see *in* the blind-spot, but, certain that the conditions for seeing must be solely in the surrounding retina, they advanced theories to substantiate their belief. Following some preliminary work done by other investigators, Dr. Helson made an elaborate study of this problem and confirmed a growing suspicion that one can see *with* the blind-spot as well as *in* it. It is too early to predict the consequences of this study, which upsets a long tradition. But it is certain that the alleged, isolated rôle played by the retina is badly in need of revision. Even the rôle of the photochemical substances cannot be understood without reference to the dynamics of the entire nerve structure from the eye to the brain.

Dr. Helson's study again illustrates functional analysis in the psychological laboratory. Note the great number of conditions that were varied during the time that others were kept under control, in order to give the prevailing hypothesis a thorough test. He found that the whole, not previously evident, is lying just around the corner.

The blind-spot has been involved in nearly every controversy regarding the eye and its functions since its discovery 260 years ago. It has played a prominent part in the discussions concerning the seat of vision in the eye, the ability of the optic nerve fibers to respond to light and electrical stimulation, facts and theories of binocular and monocular space perception, the locus of contrast and other observed visual phenomena—in short, in every moot question concerning vision.[3]

Since the relation between light and electricity, the electrical nature of the nerve impulse, and the effect of light on protoplasm have been under investigation, the problem of vision in the blind-spot assumes importance once more if it is true that the optic nerve fibers respond to direct stimulation by light or electricity. If we believe, as many biologists at the present time are inclined to believe, that all protoplasm responds more or less to light, and that the action of light on the optic nerve fibers is but a special case of the close relation existing between electrical or electromagnetic and light phenomena, then stimulation of the optic nerve fibers by light to see if excitation, conduction and sensation are possible seems to offer a promising field of investigation. If the optic nerve fibers can be shown to respond to direct stimulation by light, we shall be able to envisage the mechanisms of vision as well as the broader problems of nerve conduction and specificity of neural energies in a new manner.

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[3] Textbook accounts of the phenomena in the blind-spot hardly do justice to what is found there nor do they indicate what a large literature has been devoted to the subject. H. von Helmholtz, Treatise on Physiological Optics, trans. from 3rd German ed., 1924; A. Tschermak, Ueber Merklichkeit und Unmerklichkeit des blinden Flecks, *Erg. d. Physiol.*, 24, 1925, 330-378, have given excellent accounts.

Nor can our conclusions be devoid of practical implications ultimately because of the rôle played by the blind-spot in tests of scotoma, diagnosis of disease and a host of other ways.

That something is seen *in* the blind-spot cannot be open to question any longer in the light of many observations by numerous investigators. But even this fact has been denied by some writers. This state of affairs springs from three sources of error: (1) a very slight change in conditions, many of which are not understood, changes the observed phenomena so much that different investigators believe they have found absolutely different results; (2) hasty generalization from observations under limited conditions to the absolute capacity of the optic disk under all conditions; and (3) neglect of important and oftentimes crucial data which do not fit with one's own observations or theory.

Since practically all investigators and writers have united against Pecquet, Volkmann and the very few who have followed them in their belief that only the region of the central artery and vein is insensitive to light stimulation, we may say that while opinion is divided on the question of vision *in* the blind-spot, it is practically unanimous against the view that we see *with* the blind-spot, or in other words, that the optic disk somehow responds to light without the aid of adjoining regions. The crucial questions and the tests necessary to answer them now seem to the writer to concern direct stimulation of the optic disk. We must determine anew what is seen in the blind-spot and devise crucial tests of the hypotheses which deny any functional value to the optic disk itself.

EXPERIMENTS

General comments on the conduct of the experiments. Since the facts to be described have been matters of heated controversy and are crucial for any theory of the blind-spot the following rules or precautions have governed our procedure in conducting the experiments.

(1) Except when explicitly stated, all of the *O*s worked without knowledge of what type of stimulus was being used. *O* was left free to report whatever he observed. Questions for the purpose of eliciting greater detail in description were sometimes asked but always with a warning at the time to the *O* that he was to report only what he saw.

(2) Observations were always verified by repeated exposures of stimuli either on the same day or at some succeeding time. The *O*s were cautioned to report only what they were absolutely sure of and in cases of uncertainty further exposures or variations in conditions were made to settle the uncertainty in question.

(3) All of the facts reported here were observed by at least 2 *O*s in entire independence of each other, and, in the majority of cases, were found by all 4 of the regular *O*s. It is hardly necessary to state that the usual rule of silence regarding the experiments was maintained so that the *O*s were not influenced by each other's reports. Since 3 of the 4 regular *O*s were senior members of the staff and the fourth a graduate student of considerable attainments and seriousness, we can vouch for the integrity and independence of their statements.

(4) The objectivity of the phenomena, even though difficult to observe, is attested by the similarity of reports under similar conditions. It is not uncommon for *O*s to employ identical descriptive terms in their protocols, *e.g.,* all 4 *O*s report a single light exposed in the blind-spot as like the "searchlight of an engine shining through a fog, having a bright central core and a large halo or haze around it spreading out in all directions."

(5) Special precautions against eye-movements were taken as follows: (a) lights were exposed on the boundaries of the blind-spot simultaneously with the stimulus within the blind-spot. If the eye moved one or more of the boundary lights entered the blind-spot and underwent qualitative changes which appraised the observer of an eye-movement. (b) Stimuli in the blind-spot look so different from stimuli on the edges that the *O*s soon learned to recognize the characteristic appearance of stimuli within and to become cautious with respect to phenomena resembling peripheral or foveal vision. Thus one of the *O*s who was prone to report "flashes of clear-cut lines and forms" at the beginning of the experiments soon learned to recognize these as due to eye-movements and reported them as such. (c) Practised *O*s attain remarkable success in reporting eye-movements. Even though not all eye-movements can be reported our other criteria furnish adequate check. (d) Besides depending upon the usual devices for maintaining rigidity during observations many of the phenomena reported were observed under momentary exposures and all of them were checked in repeated exposures.

(6) While quantitative measures of all the phenomena described in this paper would have been desirable and are undoubtedly necessary eventually in order to properly evaluate our results, we were unable to obtain satisfactory measurements in the time we had for the following reasons: measurements for one *O* did not hold for another and hence did not and would not aid in duplicating results; the phenomena observed change so rapidly it would be next to impossible to correlate a given measurement of stimulus with a given phenomenon; in working with fairly large stimuli it is impossible to obtain a satisfactory measure of the energy in every part of the stimulus constellation so as to arrive at some notion of the effect of the total. Our light sources were by no means uniform nor did they illuminate uniformly the patterns cut in the screen on which the blind-spot was projected. Yet the absence of quantitative data in this study does not necessarily imply that our results are lacking in certainty, definiteness, and objectivity. For while absolute measures of stimulus are not given we have such good agreement among *O*s regarding the effects of changes in intensity, length of exposure, effects of stimuli upon each other and other general factors influencing the phenomena that we have achieved ends similar to those for which quantitative measures are desirable. Thus while we cannot state what the liminal stimulus for a given *O* is, we have found that the faintest light seen, whether we begin with a supraliminal stimulus and decrease the intensity or a subliminal stimulus and increase the intensity, and regardless of the color of filter used and its appearance in the rest of the field of vision, will be *blue*. This qualitative fact, established by a wide variety of conditions and with all *O*s makes the same claim for consideration that an unique quantitative finding would.

Methods of determining the boundaries and stimulating the blind-spot. The *O* sat facing a black cardboard screen 1 m. sq. in which a small cross had been cut to serve as the fixation point. Most of the experiments were conducted with the *O*s at a distance of 131 cm. although for some experiments the distance was changed to 4 m. Only the right eye was used in observation, the left being closed. A head-rest and biting-board made by taking a wax impression of the teeth on a metal base, all held solidly by rods and clamps to an upright bar on a heavy metal stand, served to keep the *O* rigid during observations. The *O* rested between observations, and experimental periods were not prolonged beyond the limits of ordinary fatigue in visual work.[4]

The boundaries of the blind-spot were determined in the usual fashion in a room lighted by a 150 w. incandescent lamp by bringing a white stimulus or test object (1 cm. sq. in size) into the field of vision until it disappeared while the *O* was fixated on the red cross at the left of the screen. The points of disappearance were marked along eight meridians at 45°. Only the places of *disappearance,* and not the averages of appearance and disappearance as is usually done in mapping the blind-spot, were taken as defining the blind-spot as we wished to be sure we were working well within the limits of the *macula cæca.* All stimuli exposed within the boundaries of the blind-spot were well away from the edges: when the observer sat 4 m. from the screen and we used a 2 mm. sq. light as the stimulus object, the diameter of the stimulus in this case was $\frac{1}{200}$ the smallest diameter of the blind-spot which at this distance averaged 40.63 cm. for all *O*s. Usually the stimulus was an incandescent lamp of 150-300 w. behind a ground glass in front of which the black cardboard was placed. With small stimuli or patterns cut out of the cardboard fairly sharp figures could be obtained even with the ground glass. Experiments in which the source of light was directly behind the opening in the cardboard screen gave results similar to those with the ground glass interposing. It will be seen that in the main our experiments were conducted with the usual order of conditions reversed: instead of working with the retina illuminated, we worked in general in a dark or semi-dark room and *illuminated the blind-spot,* the rest of the field of vision receiving no stimulation so far as general room illumination or the presence of other stimuli was concerned, except when we wished to test the effect of the latter upon vision in the blind-spot.

Special checks against scattering and diffusion were made in ways which will be described later.

To control the intensity of our stimuli we used rheostats in series with the incandescent lamps. For weak and small stimuli small flashlight lamps and automobile headlight lamps supplied by batteries were used. Special means of dimming our stimuli consisted in covering them with white bond paper or filters.

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[4] J. N. Evans, Angioscotometry, *Amer. J. Ophth.*, 9, 1926, 489-506, has succeeded in mapping the larger vessels of the retina. He has found two hours' mapping not excessive so far as fatigue of "fixation" goes, but stresses the possibility of "mental fatigue." We seldom worked for more than an hour with an *O*.

We varied the color of our stimuli by the use of ordinary gelatine filters pasted over the patterns cut in the cardboard screen. The light transmitted by our filters varied with the changes in intensity of our source lights, as would be expected. At medium and low intensities the green showed traces of blue while at high intensities of light it became yellowish. The blue filter was consistently blue except at high intensities when it lost so much in chroma as to be rather whitish. The red filter gave a bluish-red color at medium and low intensities but by using two thicknesses we could get a very good red from it. The amber filter was the least satisfactory, giving a reddish-brown color at low intensities and a reddish-yellow at high. The source lights without the use of filters, like ordinary incandescent lamps, were fairly yellow at medium intensities, brownish-yellow with low intensities in direct vision, and bright yellow, almost white, at high intensities. This account does not pretend to give the spectral transmissions of the filters or their specifications according to any other mode of physical analysis—we merely wish to convey how the lights *looked* with various filters at various intensities—for our purposes it is sufficient if we compare foveal and peripheral vision with vision in the blind-spot.

RESULTS: I. SIZE

The sizes of the blind-spots of our *O*s were found to be as follows: *B* (Mr. S. H. Bartley, graduate student and instructor in psychology) 5° 40′; *G* (Dr. J. P. Guilford, assistant professor of psychology) 6° 41′; *M* (Dr. Beulah M. Morrison, assistant professor of psychology) 5° 30′; and *W* (Dr. R. H. Wheeler, professor of psychology) 6° 4′. The horizontal diameters of the blind-spots of these *O*s at 131 cm. averaged 15 cm. and were slightly less than the vertical diameters because the shape of the blind-spot is decidedly more elliptical than circular. These values for the blind-spot compare well with those found by other experimenters though we must note that they are slightly smaller than if we had taken the average of *in* and *out* determinations as the boundaries of the blind-spot.

When we decided to make special test of the blur circle and diffraction hypothesis we worked within the projection of the blind-spot at 4 m. distance thereby getting nearly three-fold enlargement which enabled us to work with an extraordinarily large ratio of stimulus to blind-spot. A stimulus at 4 m. distance that measures 3 mm. on a side will be approximately $\frac{1}{35}$ the diameter of the average blind-spot (40.63) leaving as great a margin of safety for blur circles, irradiation, and scattering as could possibly be desired. In terms of area this stimulus had $\frac{1}{210}$ the area of the blind-spot (40.63 × 46.71 cm.). Such a stimulus placed at the center of the blind-spot was easily seen by all observers and in a few observations we made our stimulus as small as 2 mm. on a side with nearly as good results!

Two methods of mapping the blind-spot deserve mention at this point because of the unexpected results they gave. The fact has already been mentioned that the blind-spot shows different contours for stimuli of different colors and brightness. If, now, we map the blind-spot with the room in darkness, using as our stimulus object or ''prober'' a small automobile light carrying from 4-6 v. and encased in a black box so that only a very small beam is emitted, we find that *there is no blind-spot.* The light will be seen throughout the region previously unresponsive to our white prober when the room was normally illuminated as in our previous method of mapping. For 2 *O*s there were two tiny spots within the so-called blind-spot where the light disappeared. Another way in which to obtain the same effect is to cut a slit the length or width of the blind-spot in the screen lighted from behind and to pass another screen having a smaller slit of any desired size over the first, thus exposing a stimulus throughout the length of the blind-spot in any meridian. As the light passes the edge of the blind-spot obtained in ordinary mapping it loses its form, and shows a bright central core surrounded by a halo or haze of light which may or may not reach as far as the boundary of the blind-spot. Whether what is seen is due to stimulation of adjoining regions it is the purpose of this paper to answer.

We then reversed conditions. The room was kept in darkness as before but the whole visual field was lighted by removing our black cardboard from in front of the ground glass. This was done in accordance with a suggestion made by Professor Fernberger. A 300 w. incandescent lamp served as source and lighted the whole field very strongly. Using now a black stimulus as prober, the *O*s reported where it disappeared. The black object disappeared just as the white prober did before and we obtained approximately the same outline and area for the blind-spot as before. In this case the light which had every chance to diffuse into the blind-spot did not fill it in any sense whatsoever, because, while the *O*s did not always see the blind-spot as a gap in the field, the presence of the prober made no difference in what was seen—the opposite being true when we interfere in any way with a light that directly illumines the blind-spot when the rest of the field is dark. Sometimes, under these conditions, the *O*s did not see a homogeneous field, as Helmholtz and Werner insist is the case, but reported a gap in the field corresponding to the projection of the blind-spot. In these cases, *light on all sides of the blind-spot failed to irradiate and complete the gap in the field as required by the traditional theories.*

From the results of our mapping we may say that the size of the blind-spot depends upon the intensity of the stimulus used and the intensity of the surrounding field. When the brightness difference is in favor of the blind-spot it may be made as small as we please, even to the point where the stimulus does not disappear at all, or with some *O*s vanishes in a region so small as hardly to coincide with the true anatomical disk.

Having drilled our *O*s in holding their fixation, reporting eye-movements, winks, or any other disturbing factors, we instructed them to keep their eyes closed between observations and to open and fixate the right eye upon the red fixation spot when we were ready to expose a stimulus. When *O* had fixated and felt steady, *E* gave a ''ready-now'' signal and turned on the light which illuminated the patterns we had cut in or outside the projection of the blind-spot. The instructions to the *O*s were simply to report whatever was seen. Sometimes additional instructions called for reports on special aspects of the phenomena, such as form, color, after-images, or the like. In no case was a question asked concerning a detail until it had first been reported by an *O*, in which case the *E* asked for more information in order to keep full records and make sure of crucial points.

All of the experiments were carried on at the University of Kansas and extended over a period of two years. While only four people acted as regular observers besides the writer, several others took part in the experiments, some of them from the physiology and biology departments, others students in psychology.

II. QUALITATIVE CHARACTERISTICS

When a single stimulus, say a small bright spot 2 cm. in diam., is exposed within the projection of the blind-spot, mapped as we have described in our first method, the *O* will see a diffuse light with a brighter, denser core in the blind-spot, usually the color of the stimulus if it is of medium or high intensity. If the stimulus is of very low intensity it will change in color and finally disappear, in prolonged observation often leaving an after-image, positive or negative. Stimuli in the blind-spot lack definite form unless special precautions are taken to determine the form in a manner to be described later. The bright light seen may be described as flat, hard, or surfacy as against the diffuse halo effect surrounding it. Several *O*s described the light as being in three dimensions and shaped like a cone coming toward them.

If a very small light source is used to explore the region of the blind-spot there will be found places more sensitive than others; in one place or two the light may disappear entirely, but the area of this region of absolute blindness is so much smaller than the total area of the optic disk as to make it doubtful if the statements generally made concerning the extent of the blind-spot and its anatomical locus are true. In fact the region of blindness corresponds rather to the entrance of the retinal artery and vein than to the whole of the optic disk.

All colors will be seen in the blind-spot under the conditions of stimulation described. After-images, contrast effects, adaptation, and many other phenomena characteristic of normal vision will be found in the blind-spot, only in somewhat different degree, rather easily by the method of procedure we adopted, *viz.* illumination of the optic disk while the rest of the field is dark. At this point we wish to stress the fact that the phenomenon typically seen is a bright central core with diffuse halo, when one stimulus is exposed in the blind-spot; when several stimuli are exposed there may be several ''cores'' corresponding to the light sources, or no core at all.

III. Effect of Exposure Time

If the *O* by dint of special effort notes what is present in the first moment of exposure or what is present in a very short exposure (the latter being the shortest time in which *E* was able to expose the light momentarily—0.1—0.2 sec.) he is able to report simple geometrical forms correctly, thus showing *there is some perception of form in the blind-spot, if it can be seen before diffusion occurs.* Thus *B* and *W* reported a clear-cut homogene-

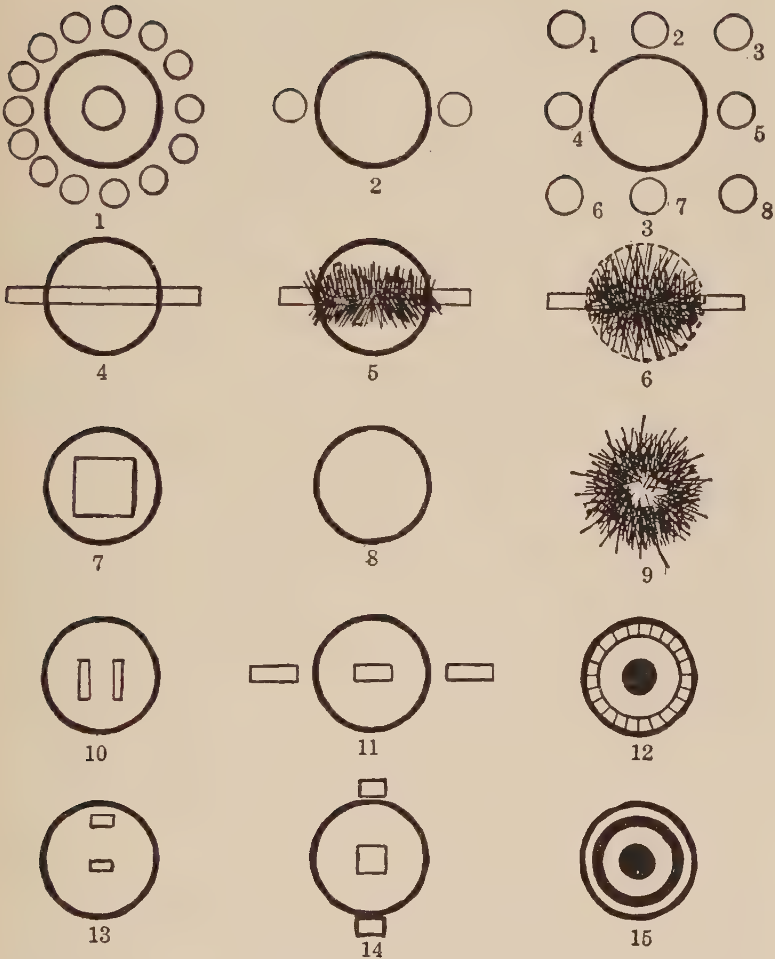

FIGURE 26, Numbers 1-15 (Original FIG. 1-15). In each case the blind-spot in the figures has been represented by the heavy black circle, although the blind-spot itself has no such regular form. The representations of the stimuli inside or outside the blind-spot are not always in correct ratio to the size of the blind-spot in these figures; however, the proper dimensions are given in the text, to which the reader is referred.

ous bar of light going through the blind-spot when a horizontal rectangle (26 × 2 cm.) was exposed so that it passed through the blind-spot (Fig. 26, no. 4). After the first fraction of a second in which the form was seen, these *O*s reported that the center portion (the part falling in the blind-spot) began to waver, grew diffuse (Fig. 26, no. 5) and finally became blurred (as in Fig. 26, no. 6). When a square was exposed wholly within the blind-spot, *B* reported that it was first seen as a square which rapidly changed to a circle, and then became a diffuse brightness with a brighter central core, as shown in Fig. 26, Nos. 7, 8, and 9. All observers agree that as time goes on whatever distinctness of forms the stimulus may have had is lost and the light gradually grows less bright and changes in color. The last color seen with continued observation (*Dauerbeobachtung*), as one adapted to the stimulus, regardless of what color filter was used, was *blue*. This is also true of the weakest intensity of light visible in the blind-spot, as reported in the section concerning changes in intensity of stimulus.

When the stimulus has completely faded out in long exposures, after-images often appear. In such cases the *O*s reported that the stimulus light had been changed. Two *O*s (*B* and *W*) often expressed uncertainty in their protocols as to whether the stimulus had been changed or they were seeing after-images.

Two other changes, besides loss of form, in time are important. When the light is first flashed on the stimulus expands from a small light to a maximum and quickly shrinks in less than a second to the size characteristic of its intensity and objective size. These three phases, really constitute what has been named *gamma* movement. After these initial phases the light gradually decreases in size and brightness, obviously a result of adaptation.

Adaptation occurs very much more rapidly in the blind-spot than elsewhere in the field of vision. Not only does the light seen decrease in size and brightness in continued observation but undergoes changes wholly analogous to the changes observed as the intensity of light is decreased. Green goes directly to blue as the *O*s adapt, yellow becomes more reddish and finally blue, amber becomes more brownish-red and finally blue. The last color seen either as a result of adaptation, or of decreasing the intensity of the stimulus from noticeability to unnoticeability or increasing the intensity of the stimulus from unnoticeability to noticeability, is blue. The fact that Ferree and Rand found the blind-spot to be smallest when mapped with a blue stimulus has been noted above.

IV. Effect of Intensity

The effect of changes in the intensity of the stimulus on phenomena in the blind-spot was next considered.

Procedure. The intensity of the stimulus light exposed in the blind-spot was varied by means of rheostatic control of the current supplying the sources of light. The *O*s reported changes as the stimulus was changed from dim to bright and from bright to dim. Red, green, blue and amber filters were used.

Results. At the weakest intensity, regardless of the color of the stimulus in foveal vision, the light in the blind-spot was seen as blue. Typical reports like the following substantiate this generalization.

No filter; *E* decreases intensity of stimulus; *O* reports: ''At first a yellowish white light which gets redder and redder, then an azure blue, shrinking in size at the same time.''

Green filter; procedure as above: ''A green light which gets bluer and bluer until there is no green left in it.''

Amber filter; procedure again like above: ''The light was yellowish-red at first, then became redder, then blue at the end.''

With the blue filter decreasing intensity changes the size of the light seen, with only a darkening of hue at the lowest intensity.

If the stimulus is exposed at the lowest possible intensity and gradually is increased, the reverse order of appearance of colors with each filter is found. All filters look blue at the lowest intensity. The amber filter changes from blue to red or orange and only at high intensities does it appear with sufficient yellow in it to be called amber. The red filter begins as blue, although it looks a good red in direct vision, turns to red and gains in yellow as the intensity is increased. The green filter requires less light in order for the green component to become visible. At highest intensities the green filter looked yellowish.

We next compared the order of colors as the intensity of stimulus changed when the stimulus fell within the blind-spot and when it fell on the edge of the blind-spot. Another fixation light was so placed that by moving the eye from the usual fixation-point the stimulus could be made to fall upon either the right, left, top or bottom edge of the blind-spot. It was thus possible to compare what was seen in the blind-spot at any given intensity with what was seen on the edge immediately contiguous to it. The results are most important for those theories that assume what is seen in the blind-spot is due to stimulation of neighboring retinal elements. *For a stimulus of given intensity which appears in the blind-spot as blue appears on the edge of the blind-spot as bright yellow and whereas the color of all stimuli at lowest intensities in the blind-spot is generally blue, on the margin of the blind-spot it was found to be yellow with no filter, and orange with the amber filter.* On the basis of the scattering theories of vision in the blind-spot we should expect the first visible light on the edge of the blind-spot to be the same as the first visible one within, but this does not happen. Nor can it be said that what is seen in these cases in the blind-spot is an after-image because; (1) it holds for the first stimulus seen as the intensity of the stimulus is increased; (2) the colors seen do not necessarily conform to the color of the negative after-image, nor is it the positive since the intensity of the stimulus is on the increase; and (3) obliteration of the stimulus light may be followed by an actual after-image, different from the previously reported light, or by no after effects at all, showing that what was seen does not fall under the category of those phenomena classed under persistence of vision.

The effect of change in intensity of stimulus upon size of the light seen in the blind-spot has already been noted. [Section omitted.] It was pointed out that due to spread of light in the optic disk all stimuli tend to be seen as diffuse patches of light larger than the stimulus. Since we should expect the spread or diffusion of light to be proportional to intensity we have an explanation for the generalization that the greater the intensity of stimulus, the larger the patch seen in the blind-spot. With increase in intensity go also an increase in size and brightness as well as the changes in color noted above.

But the intrinsic brightness of the stimuli falling in the blind-spot is not alone of importance in what is seen there. If observations are made in a lighted room, rather than under conditions of darkness as most of our experiments were made, then the effect of lighting the rest of the retina is equivalent to decreasing the intensity of the stimulus in the blind-spot. For the brighter the room light (or the rest of the retina), the brighter must be the stimulus falling in the blind-spot in order to be seen. Thus a stimulus which can not be seen in the blind-spot while the room light (150 w. incandescent lamp, shielded from the *O*'s face) is on, will begin to emerge in the blind-spot more and more strongly as the room light is turned down.[5] It will attain its maximum brightness and size when the room is completely darkened. *Maximum effects in the blind-spot are obtained when the difference in brightness between the blind-spot and the rest of the retina is a maximum and in favor of the blind-spot.*

If two stimuli are exposed simultaneously within the blind-spot, only the stronger, or in some cases the one having favored position, will be seen. But if the intensity of the two be made equal, then both may be seen at once, or rivalry may set in (see Section V).

In concluding this section the fact must be emphasized that all phenomena in the blind-spot depend in large measure upon the intensity of the stimulus, the state of adaptation of the eye, and the degree of illumination of the field outside the blind-spot.

V. DOMINANCE, RIVALRY, AND FUSION

The effect of different stimuli, both within and without the blind-spot, upon each other was next considered.

Procedure. In this series of experiments more than one stimulus was exposed at a time, either two in the blind-spot or one within the blind-spot and one or more outside the blind-spot near one of its edges. All combinations of the colors red, green, blue, amber, and yellow, taken two at a time, were used.

Results. Just as in normal vision when several stimuli are exposed simultaneously in the visual field, one of four things may happen: (1) all the stimuli may be seen simultaneously; (2) first one stimulus may be seen, then another, in rivalry; (3) one stimulus may dominate, obliterating the other so completely it never appears; (4) or the stimuli may fuse. All of these possibilities were realized with all observers in the blind-spot.

Dominance. This was the easiest to get because if either stimulus was stronger than the other, whether within or without the blind-spot, the other did not appear; although it must be borne in mind that if the stimulus in the blind-spot is to be seen as against the one on the edge it will have to be a great deal more intense than if the one on the edge is to dominate over the one in the blind-spot. This is in accordance with our law that if anything is to be seen in the blind-spot the differential brightness must be in favor of the optic disk. Some typical results follow.

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[5] Feinberg, *op. cit.*, *Psychol. Forsch*, 7, 1926, 16-43, reports a similar finding and gives some good illustrations of the effect of general illumination on what is seen in the blind spot.

B reported only the blue when blue and red rectangles (23 mm. × 3.5 mm. with 45 mm. between) were exposed within the blind-spot (Fig. 26, No. 10). Nor was the red seen in this case after the blue had adapted out and nothing was seen. This was true of all the *O*s until careful adjustment of the intensities made it possible for them to see both simultaneously.

G reported only the two yellow rectangles on either side of the blind-spot when they were exposed with a blue rectangle inside the blind-spot (Fig. 26, No. 11). Domination of the stimulus in the blind-spot over the stimulus outside was obtained by making the blue in the blind-spot very intense and an annulus of yellow around the blind-spot very weak (Fig. 26, No. 12). *O* reported a very dark navy blue with almost no trace of the ring outside in the last case, which shows that a stimulus inside the blind-spot may dominate over one outside.

M and *W* report domination of one stimulus over the other when both are within the blind-spot (Fig. 26, No. 13). In this instance the stimulus falling in the center of the blind-spot was effective and the addition of another stimulus near the top edge of the blind-spot made no difference in what was seen. If, however, the top stimulus was exposed alone it was seen, but as soon as the center stimulus was added it blotted out the top one. It should be borne in mind that here the ''top'' stimulus was 15 cm. from the nearest edge of the blind-spot while the central one was 25 cm. and hence, on the irradiation and scattering theories, the ''top'' one should have been preferred since the amount of light affecting neighboring sensitive retinal elements is inversely proportional to the distance from them.

A somewhat different type of dominance is that reported by *W*. Three squares were exposed as in Fig. 26, No. 14, one above the blind-spot, the second in the blind-spot and the third below the blind-spot. When all three were exposed simultaneously the one in the blind-spot was not seen at all. But when either the top or the bottom stimulus was obliterated the one in the blind-spot appeared. But when the third square was added to the other two, the middle one disappeared.

Rivalry. Under conditions described above, a more or less regular alternation of stimuli may occur, thus when red and green (Fig. 26, No. 10) were exposed within the blind-spot first one was seen and then the other. By making now the one color more intense, now the other, either could be made to obliterate the other.

A good description of the fluctuations is the following given by *G* when a blue was exposed in the left half of the blind-spot and a red rectangle in the right half as shown in Fig. 26, No. 10: "First a good rose red at right hand side, not very large and not very dense. Then a big patch of blue loomed up more to the left with a strong dense center and a lot of diffusion to the left. Red disappeared for a moment and came back at the right. Several minor fluctuations in which first blue would appear and then red as in retinal rivalry."

Similar descriptions of rivalry within the blind-spot were given by all the *O*s.

Fusion. Occasionally when two differently colored stimuli were exposed simultaneously within the blind-spot they fused and were seen as a mixture. *G* reported when red and green were exposed that he saw a gray. This is the only case of fusion involving complementaries with a resulting gray. Sometimes when there is not a good dividing line between the two colors they may merge into each other, thus red and yellow gave orange either over the whole field or in the region between the two stimuli, while the red and yellow were seen as such beyond the orange region.

VI. Effect of Dual Stimulation

Dual stimulation was most difficult to obtain due to the fact that if either stimulus is brighter or more *Eindringlich* for any reason it tends to dominate or rivalry sets in. With careful adjustment of the intensities of the two stimuli it was possible, however, for all *O*s to see *two* colors at the same time when both were exposed within the blind-spot.

Procedure. Different pairs of colors, red and green, green and blue, blue and yellow, red and yellow, yellow and green, and red and blue were exposed within the limits of the blind-spot as shown in Fig. 26, Nos. 10 and 15. The stimuli were either rectangles cut in the cardboard screen 23 mm. × 3.5 mm. with 45 mm. between, illuminated from behind, or two concentric rings of different color. The *O*s were not aware of the colors or of their position before they appeared. The position of the colors was changed, one appearing on the right, above or below the other. *O* reported not only on the color but its position and relation to the other, if both were seen.

Results. All the *O*s were able to see two colors simultaneously within the blind-spot. A typical description of the phenomenon, reported by *G*, follows: "I saw two colors, red at the right and green-blue at the left. There was a good dividing line between them, *i.e.*, where one left off and the other began."

We next utilized a test suggested by Professor K. M. Dallenbach and Dr. W. T. M. Forbes of Cornell University as crucial for the theories of irradiation and scattering: two concentric rings, both within the blind-spot and of different colors (see Fig. 26, No. 15) were exposed. In this series of experiments the blind-spot was 20 × 27.5 cm., the red ring having a diameter of 12 cm. and 1 cm. wide while the inner ring was blue having a diameter of 4 cm. and was solid. There was therefore a distance of 7 cm. between the inner color and the surrounding one and 8 cm. from the edge of the outer ring to the nearest edge of the blind-spot. In this procedure the outer concentric ring acted as an envelope for the inner and if they were both seen at once it is inconceivable on the basis of the irradiation theories how the inner could be seen without breaking through its envelope. Under these conditions 3 *O*s (including the writer) *saw both colors, simultaneously, one within the other:* one *O* was uncertain, and a fifth was unable to see both. In order for this experiment to succeed it is necessary to control the intensity of each color and to vary it until the proper intensity is obtained so that neither will dominate. The space between the two colors helps eliminate fusion. Momentary exposure may aid some *O*s to see both colors clearly simultaneously.

VII. PERCEPTION OF APPARENT AND REAL MOVEMENT

The perception of movement, apparent and real, in the blind-spot is now considered.

Procedure. A small light, 2 mm. sq., was moved about in the projection of the blind-spot as the real moving stimulus. For the production of apparent movement (stroboscopic or *phi*-movement) two methods were employed: (1) we found that if one stimulus was exposed briefly and then a second exposed in another part of the blind-spot it was possible to produce apparent movement, although in this simplified procedure it is not possible to produce movement with the regularity and smoothness we have come to expect as a result of the excellent methods developed since Wertheimer's classic study; (2) by cutting two holes in the screen on which the blind-spot was projected and by rotating a disk behind them in which slits had been cut so as to coincide with the holes in the screen it was possible to regulate the time of exposure of each stimulus in the blind-spot, the time and distance between stimuli and the intensity of each. By this latter means apparent movement from stationary stimuli can be produced at will, the direction and character of movement being independent of the direction of rotation of the disk since it is seen equally well in a direction opposite to that in which the disk revolves.

Results. Real movement is perceived by all the *O*s in the blind-spot. Stationary stimuli exposed in rapid succession by the first method produced apparent movement for *G*. Since this *O* had been observing in a problem on the *phi-phenomenon,* he may have been "set" for it and hence perceived it under conditions that were not optimal for it. Yet *G* was surprised to find movement under these conditions since we were at work upon another aspect of the problem. We thereupon repeated our procedure with another *O* (*W*) who also reported movement, but he was not so sure about its sensory character, feeling that he may have "inferred" it. Our purpose in mentioning these two cases here is to show how easy it is to produce apparent movement in the blind-spot, even with such crude procedure as is involved here—procedure which would not suffice in direct vision. But other investigators have also pointed out the fact that the *phi-phenomenon* occurs even more readily and smoothly here than in other parts of the visual field.

With apparatus actually designed for the production of apparent movement, as described in the second method given above, we were able to substantiate the findings of Stern[6] and others that movement may be seen in the blind-spot (1) when both stimuli lie on either side; (2) when one stimulus is within, the other without the blind-spot; and (3) when both stimuli are entirely within the blind-spot. All the *O*s have no difficulty in seeing apparent movement in the blind-spot when the conditions are at all favorable.

The perception of apparent movement within the blind-spot when both stimuli lie within it is most significant for any theory of vision in the blind-spot. For it is inconceivable on the basis of the scattering and irradiation theories how light radiating from different parts of the optic disk to surrounding retinal elements could give rise to a movement process unseen in the sensitive regions and then localized as in the blind-spot. According to the irradiation theories we are required to assume not only that single *points* falling in the blind-spot are seen in the blind-spot through their effect on neighboring retinal points but, what is far more difficult to believe, that the retinal points stimulated give rise to a movement process over a retinal *area* (or line) unseen in the places actually excited but localized as in the blind-spot. The same difficulties will be encountered if, *mutatis mutandis,* the process is supposed to take place in the cortical projection of the retina. It was this same difficulty of localization in the irradiation theories which forced Helmholtz and others to give them up in part. In the face of these and other findings they must be given up *in toto.*

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[6] A. Stern, Die Wahrnehmung von Bewegungen in der Gegend des blinden Flecks, *Psychol. Forsch.,* 7, 1926, 1-15.

VIII. PERCEPTION OF FORM

In general, especially after the first fraction of a second of exposure of a stimulus, we may say that perception of forms in the blind-spot is either poor or wholly lacking. The usual thing seen is a haze spreading out in all directions, regardless of the form of the stimulus. But differentiations in brightness within the haze may lead the *O* to infer that certain figures are present. Thus triangle, circle, square and rectangle seem different even though the *O* cannot say just what the form of each may be. There are, however, two notable exceptions to the general statement that form perception is either lacking or poor in the blind-spot: (1) when the *O knows* the shape of the stimulus exposed he is able to assert that differentiations in brightness within the total field suggest what is actually the form of the source; (2) but with very brief exposures or with extreme care on the part of the *O* during the first moment of stimulation, the form of the stimulus is actually seen and not inferred. Forms may be seen with sufficient distinctness to be recognized and named correctly. After the initial *Einprägung* the form evaporates, as it were, into the diffuse phenomenon described above. Even though the process of diffusion *within* the optic disk takes place rather quickly it is still not so rapid but that the *O* can detect the form before diffusion has taken place and can actually see the change from a more or less clearly bounded figure to a large shapeless mass possessing more or less brightness differences within. One *O* (*W*) is positive that form is given *perfectly* at the beginning of stimulation, but that its rapid disappearance makes it hard to detect.

Procedure. Various figures at different intensities were exposed in the projection of the blind-spot with both short and long exposure times, with and without knowledge. The *O*s were required to report on the form of the phenomenon seen and to note any changes it underwent in time.

Results. Typical results for each *O* are given, (1) observing without knowledge and then (2) observing with knowledge.

(1) *Without knowledge.* *B* reported a half moon exposed in the blind-spot as "a ring effect, not fully complete" (Fig. 27, No. 17). Two squares, one above the other, were reported as an upright rectangle (Fig. 27, No. 18). In this case diffusion occurred *within* the blind-spot from one figure to the other over a distance greater than the distance from either stimulus to the nearest edge of the blind-spot, the total phenomenon being localized within the boundaries of the projected blind-spot. A cross exposed in the blind-spot (Fig. 27, No. 19) was called a cross but its form was greatly distorted as compared with the stimulus. It appeared as shown in Fig. 27, No. 20. Six small circles exposed in the blind-spot were reported as giving rise to a less homogeneous field than three spots (*cf.* Fig. 27, Nos. 21 and 22).

M reported a triangle exposed with the base uppermost as a vertical slit with the lower end narrower than the upper. A number of slits forming an eight-rayed star were reported as differentiated, not round (Fig. 27, No. 23). A cross appeared not exactly round but *M* could not say what shape it was. The two squares in Fig. 27, No. 18 appeared as *two* spots for *M* in contrast to the single figure *B* reported.

W reported that a half moon was different from a cross, but could not tell in what respect. Multiple cores, as for *B*, are distinguished from one or few cores (Fig. 27, Nos. 21 and 22).

(2) *With knowledge.* *G* and *W* looked directly at a blue rectangle and then turned the eye so that it fell in the blind-spot. Both reported that form was preserved even in the blind-spot, although not as definite and clear as in foveal vision.

G describes the experience as follows:

First I looked at fixation-point and got a blue patch of light in the blind-spot, rather small, formless, all a film color, no hard surface, and more dense in the center and much smaller in area than the blind-spot. As I turned my eye slightly off the fixation-point toward the blue stimulus, about two inches from the fixation spot the blue shrank in size, became smaller and elongated vertically and then seemed to be less blue

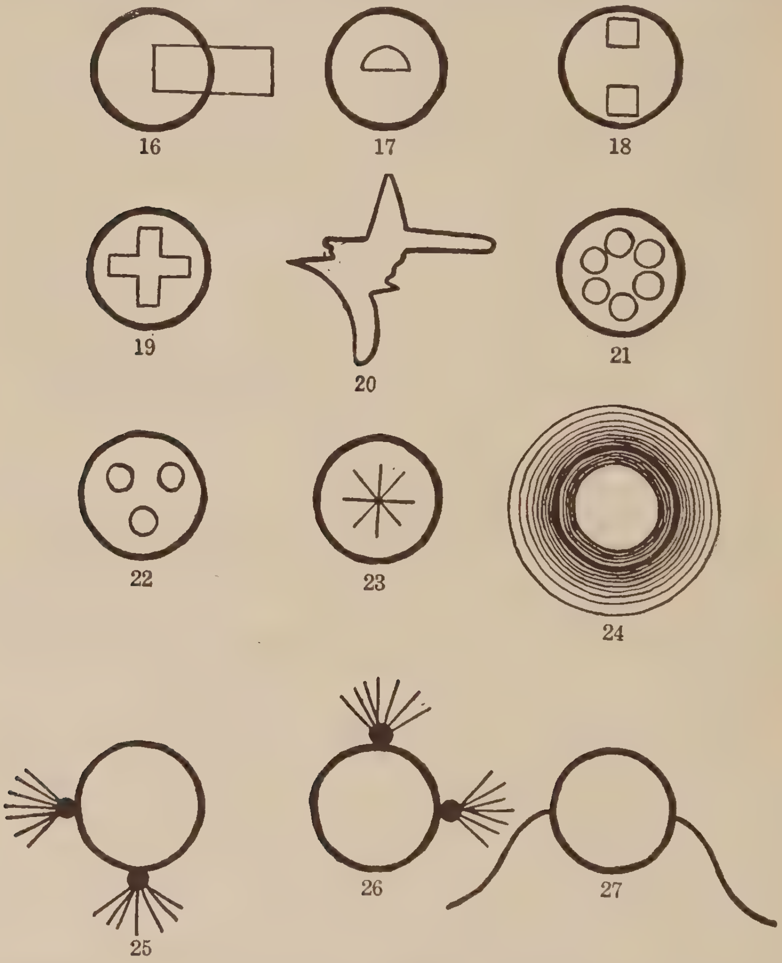

FIGURE 27. Numbers 16-27 (Original-FIG. 16-27). For explanation see legend to FIG. 26 and the text.

and dense. Going farther it took its definite rectangular shape and got more surfacy and definite until I fixated it directly. Going back the changes took place in reverse order. When seen at the time my fixation was about two inches from the fixation point it seemed less blue than at the fixation point itself. After I knew the shape of the thing, while fixating the red spot (*i.e.*, while stimulus fell within blind-spot again) it retained its elongated form.

W's reports show much more definite preservation of form in the blind-spot when working with knowledge.

After looking directly at the blue slit and then turning fovea to the red fixation light the blue slit kept its form.

W was unable to find a place in peripheral vision where an intense blue stimulus lost its form to any appreciable extent, showing that the amount of scattering of light after reaching the retina is so small as not to be perceptible, hence the diffuse nature of the phenomenon seen in the blind-spot is due not to scattering, but to the lack of receptors responsible for the perception of form and detail.

Short exposures. All the *O*s agree that at the very first moment of exposure form is present though it may not last long enough to be definitely identified. *M* often reported "at the very first there was a suspicion of form." *W* was most successful of all in reporting form after very brief exposures of stimulus. He succeeded in naming and drawing correctly the following figures which, however, appeared "fuzzy" in various parts and lacked the distinct outline we are accustomed to find in good vision: triangle, half moon, cross, and rectangle. Probably most of the successes with forms in the long exposures were due to having seen them at the beginning.

What slight perception of form exists in the blind-spot furnishes conclusive evidence against the theories which assert that vision in the blind-spot is due to excitation of end-organs in the adjacent retina.

IX. EXPERIMENTS DESIGNED TO TEST ALTERNATIVE THEORIES OF VISION IN THE BLIND-SPOT

Most of the theories concerned with vision in the blind-spot take no account of the possibility that the blind-spot may respond to direct stimulation, or at least disregard what is seen there under such conditions. Theorists have been engaged in explaining why there is no gap in the field of vision or why the optic disks may appear in contrasting colors to the rest of the field. Our experiments raise the problem as to how it is possible to see *with* the optic disk, a place where, it is assumed, receptors are lacking for the transformation of light into energy suitable for conduction along the nerves. Only two theories have been advanced in the literature to explain vision *with* the blind-spot: (1) that the light seen in the blind-spot is due to eye-movements causing the stimulus to stimulate contiguous retinal areas, advanced by Helmholtz to explain the fact that some of Donders' patients saw the image of a flame when thrown directly upon the optic disk; and (2) light directed to the optic disk scatters and falls upon neighboring parts of the retina, but it is assumed that what is seen is localized as coming from within the blind-spot. The authors of the contrast theories have not considered the possibility of direct stimulation of the blind-spot with resulting sensations of light and so their theories reduce to explanations in which the optic disk plays a wholly negative rôle, adjoining regions of the retina, or their central projections, being responsible for what is seen in the blind-spot. We shall therefore have to consider theories which *may* be advanced to explain our results, as well as theories already formulated since, with one exception, no thorough-going piece of work involving direct stimulation of the optic disk has yet been published.

It must be borne in mind that *evidence may be found to support any theory of vision in the blind-spot if contrary evidence is neglected*, as has been done all too often in the history of the problem. We have therefore selected those experimental tests which we believe to be crucial for all hypotheses which do not admit the possibility of direct response of the optic nerve fibers to light stimulation. Of the hundreds of experiments made and contemplated most seemed to be inconsequential so far as being definite tests of alternative theories. Hence only those tests will be given and discussed which seem to have a decisive bearing on the fundamental issues.

Series 1. The *O*s fixated on a lighted ground glass screen, illuminated from behind. The projection of the blind-spot (which we found by previous mapping) was then covered with a black cardboard. *There is no filling in of the blind-spot,* although light is stimulating the retina immediately contiguous to the optic disk, nor does the light take on the form of the blind-spot. In fact one observer (*G*) reports seeing clearly a dark disk in the place occupied by the blind-spot. Under these conditions no *O* reports the bright central core with surrounding haze or halo, so characteristic of vision in the blind-spot.

Series 2. Better than the above, but a modification of it. Instead of lighting the whole field around the blind-spot, we exposed a bright light in the projection of the blind-spot, and placed a piece of cardboard so that the only light present was that due to scattered light around the edges of the card. A faint haze was seen by all the *O*s, *but no O reported the bright central core.* In other words, when we have a gradient of brightness, greatest at the edges of the blind-spot and diminishing as we go away from it, the blind-spot is not filled in in the sense of the gradient as we should expect if the theories of scattering and irradiation were true. If we uncover the stimulus (so placed as to stimulate the blind-spot directly in the center) all *O*s immediately report the bright central core; and if we move the stimulus in the blind-spot the bright central core moves in a manner corresponding to the actual position of the stimulus.

Series 3. The above experiment more rigidly controlled. Mapping the blind-spot on screens at various distances, we obtained a series of projections increasing in size as we increased the distance. We then cut out the projection of the blind-spot in each screen, taking care that each map should be in alignment with the rest by special tests. We thus have a series of diaphragms to take care of scattered light. This procedure we believed was better than the use of a cone from eye to screen since the inside of a cone even though black would act as a multiple reflecting surface, giving scattered light rather than eliminating it. Under these conditions a weak stimulus was seen as a bright core with halo but if a small piece of black cardboard 1½ the diameter of the blind-spot was placed in front of the light source, *the bright central core disappeared immediately,* although one half of the blind-spot was still stimulated by light. The core seen in the blind-spot corresponds to the brightest part of the source since covering that up caused the core to disappear. Even though the conditions postulated by the scattering theories were fulfilled in stimulating the periphery of the blind-spot no bright core was seen as we should expect if they are true. Nor did the light on the edges of the blind-spot take on the outline of the optic disk as we might have expected from the alternative theories. Since what has been said held true for the most intense stimuli (300 w.) used as well as for the weakest, our results cannot be explained as being due to insufficient stimulation. In using 300 w. light sources at 131 cm. from the *O* we actually obtained more light on the periphery of the blind-spot than would have been possible in the majority of our experiments carried on with weaker stimuli. Thus while we took special pains to fulfill the conditions of the irradiation and scattering theories the expected did not happen. We must conclude that the bright central core seen in the blind-spot under direct stimulation is due to the brightest part of the source, moving with it and disappearing when it is obliterated. Neighboring portions of the retina cannot be responsible for the bright core since no core is seen when the amount of scattered light is greatly increased but direct stimulation is absent.

Series 4. Still fulfilling conditions of the scattering and irradiation theories by varying *Series 3* as follows: we made rings of increasing brightness toward the boundaries of the blind-spot, thus actually having a gradient of brightness whose maximum was at the boundaries of the blind-spot. In some cases we even extended the rings into the blind-spot 2 cm. (see Fig. 27, Nos. 24 and 27). Under these conditions, with light actually streaming in on all sides of the blind-spot and growing brighter as they approached the center, *no bright core appeared.* The completion in gradient of brightness in the blind-spot demanded by the scattering and irradiation theories did not take place even though, as here, optimal conditions were made for them. In fact we made conditions more in accordance with these theories than they were in the rest of our experiments, yet no bright central core appeared in the blind-spot, *one observer even perceiving a gap in the field where the rings ceased.* In some cases, even when the blind-spot was permeated by a haze from the rings extending into the blind-spot, no brighter core was present.

Series 5. Comparison of scattering in various parts of the retina and the blind-spot. It is generally conceded that the amount of scattered light is proportional to the intensity, and, we may add, to the length of fixation, increasing as the stimulus is fixated. Special experiments were made with *G, M* and *W* to determine whether the amount of visible scattering changed in different parts of the retina, especially near the borders of the blind-spot. While stimuli change in tint and chroma in passing from the fovea to the periphery there is very little difference in the amount of perceived scattered light as such since there is not enough to cause appreciable distortion of form. One *O* (*M*) found considerable difficulty in getting clear-cut forms in the periphery but reported no visible irradiation. Stimuli bordering the very edges of the blind-spot are seen as fairly sharp lines and do not diffuse into the blind-spot.

On the basis of the scattering theories we should have to assume that there is a sudden increase of scattering of light in the eye in one place—the region around the blind-spot. Moreover, the amount of scattering necessary would have to be so great to account for what is seen in the blind-spot that it passes belief, for normal vision with sharp contours could never then be possible. That scattering does not occur until we reach the inside of the blind-spot is proved by our experiments in which stimuli exposed on the edge of the blind-spot preserved their form. The diffuse halo in the blind-spot is due, then, not to scattering of light from the blind-spot to the adjoining regions and back again (irradiation theories require the transportation of light both away from and back to the optic disk) but to the nature of the optic disk itself which is composed of naked axones which are probably good refractors of light. The lack of receptors in the blind-spot would probably explain the lack of form perception.

Series 6. We know that stimuli falling within the blind-spot tend to spread or diffuse. The direction of spread can be reported upon by the *O* so that if we expose two stimuli within the blind-spot at different distances from the edges, we should expect, on the basis of alternative theories, the one nearest the edge to be the more intense, since the scattering is inversely proportional to distance from sensitive elements. We exposed two stimuli 10 cm. from the nearest edge of the blind-spot and 20 cm. from each other to fulfill the distance condition of the scattering theories. Assuming that what is seen is due to stimulation of retinal areas adjoining the blind-spot by scattered light, we should expect the brightest parts to be near the edges of the blind-spot since the stimuli are nearer the edges than to each other. Under these conditions, when both stimuli were of equal intensity, they were seen as a band of light extending from one to the other, the diffusion taking place *within* the blind-spot and not out toward the edges which were nearer. Moreover, the whole phenomenon was confined to the blind-spot, nothing being seen at the edges. Again, when we exposed a stimulus in the center of the blind-spot and added another stimulus of the same size and intensity above it and near the top edge of the blind-spot, the addition of the second made no difference to what was seen (Fig. 26, No. 13). When we exposed the top stimulus alone it was seen, but as soon as the central one was exposed it obliterated the top one, although the latter was only 5 cm. from the nearest edge of the blind-spot while the center one was 19 cm. Since the effects described do not conform to what we should have expected from a scattering mechanism we must conclude that what was seen was due to direct response of the optic fibers stimulated or to scattered receptors in the nerve-head.

Series 7. Experiments designed to determine if stimuli irradiate into the blind-spot. The *O* moved his eye so that a stimulus fell now on one side of the blind-spot, now on another (Fig. 27, Nos. 25 and 26). In such cases there was no visible irradiation of the stimulus into the blind-spot, not even when the stimulus extended part way into it. Not until a large part of the stimulus falls into the blind-spot is it seen at all so long as stimulation is present on adjoining retinal areas which inhibit vision in the optic disk. We have already mentioned the fact that stimuli preserve their form right up to the edges of the blind-spot and hence scattering does not occur in a perceptible degree. Moreover, if an annulus of yellow surround the edges of the blind-spot and a blue is exposed inside, there will be no interference of the one with the other, both being visible simultaneously. Indeed, a dark space may be seen between the annulus and the stimulus within the blind-spot showing irradiation does not extend *outside* the boundaries of the blind-spot nor *into* them.

Series 8. Simultaneous exposure of stimuli near the boundaries of the blind-spot and within the blind-spot. It is of great importance in these experiments to get the proper intensity of stimulus within the blind-spot, to verify the facts reported here. All combinations of the colors red, green, blue, yellow, and amber were used. The facts which emerge as of the greatest importance in this series of experiments are that: (1) when the blind-spot is completely surrounded by an annulus of color and the center is stimulated with the complementary color (Fig. 26, No. 12) both may be seen simultaneously with no encroachment of the inner on the outer or *vice versa*, when the intensity of the inner is made just sufficient for visibility and no more. In fact, *O*s report a dark space between the two stimuli. (2) Stimuli on either side of the blind-spot (Fig. 26, Nos. 11 and 14) do not interfere with what is seen in the blind-spot if the intensity of each is regulated. It is possible to increase the intensity of the stimulus in the blind-spot until its haze extends beyond the boundaries of the blind-spot. But even in foveal vision intense stimuli fixated for a long period in the dark tend to diffuse over the retina, even blotting out weaker stimuli in the rest of the field. The blind-spot is therefore not an unique region so far as the presence of haze is concerned, here it appears much more readily but stays within the boundaries of the blind-spot if the stimulus is not too intense. Again, we find the blind-spot comparable to the rest of the field of vision, different only in degree. (3) Stimuli right up to the edges of the blind-spot (Fig. 26, No. 11) maintain sharp outlines and are not seen in the blind-spot unless they extend far into it, or special care is taken that they shall be seen by increasing the intensity of the part within. Moreover, as soon as stimuli become visible in the blind-spot their qualitative character is so different from that which they bear outside that there can be no question different mechanisms are responsible for what is seen in the blind-spot from those functioning in normal vision. Thus a stimulus falling partly within and partly without the blind-spot as in Fig. 27, No. 16, was bright yellow outside and reddish yellow inside, the outer portion retaining its form on the very edges of the blind-spot. (4) Stimuli bordering opposite sides of the blind-spot (Fig. 26, No. 12) are not necessarily completed within the blind-spot by irradiation as so many writers assume; indeed, a gap may be seen between them corresponding to the position of the blind-spot.

Series 9. In this series of experiments the blind-spot was mapped at a distance of 4 m., giving an average horizontal (small) diameter of the blind-spots of our *O*s of 40 cm. while the stimulus consisted of a small square slit, 1 cm. on a side. Thus the stimulus or image on the retina was only 1/40 the diameter of the blind-spot or total optic disk. By bringing a card over the stimulus from one side or making it smaller from all sides, it was possible to cut it down to a point. If the core is due to scattered light, then the less scattered light present the dimmer and smaller should be the core. All the *O*s agreed that the effect of cutting down the stimulus was to reduce the size of the haze or halo, *the core being the last to change and go.* When the core disappeared, it went suddenly, taking the halo with it, or what remained of the halo. This experiment is the reverse of experiments in *Series 2, 3,* and *4,* where the core was deleted by obliterating the brightest part of the stimulus while the edges of the blind-spot were illuminated from the rest of the source. Here we obliterated the light from the edges to determine if the core was due to light coming into the blind-spot from without, with the negative result reported. Similar experiments in which the "cutting off" of light was made at the eye by bringing a card across the pupil gave identical results as cutting off at the source. It thus appears that the core is the cause of the halo while it itself is due to the direct action of the stimulus upon the optic nerve fibers or some mechanism present there capable of acting as end-organs.

Series 10. Since the perception of two colors within the blind-spot is crucial so far as alternative theories are concerned, we mention the fact again here that two colors, exposed side by side or in the form of concentric rings within the blind-spot or as annulus around the blind-spot while the other is within, can be seen simultaneously—a definite disproof of the scattering and irradiation theories.

THEORETICAL AND CONCLUDING REMARKS

If definite meanings are given to such terms as scattering, irradiation and the like, all of which have been assumed to explain vision in the blind-spot, then we may devise tests to verify them. We expect certain phenomena to be present under certain conditions and absent under others if these words have assignable meanings. Thus we expect the effect of scattering to be inversely proportional to the distance from the places affected by it and we can see if a stimulus near sensitive retinal elements is seen better in the blind-spot than one farther away. By a systematic variation of conditions we tested alternative theories of vision in the blind-spot and found that what we expected did not happen while in many cases what did happen is unexplainable if we accept these terms as we now understand them.

When, as in our experiments with a 2 mm. stimulus at 4 m. distance from the *O*, the image on the disk is only 1/200 the total area of the disk, the amount of scattered light reaching the neighboring retina must have been vanishingly small. Moreover, since the haze from this stimulus did not fill the blind-spot we should have to assume that a subliminal stimulus at the edges of the blind-spot can cause a supraliminal effect in the central region. This difficulty entails another—that of eccentric projection—for if surrounding rod-cone elements are responsible for what is seen in the blind-spot, why is not the stimulus localized on the edges as it is when we actually stimulate the boundaries of the blind-spot with an annulus of light? Furthermore, if the amount of scattering, diffusion and irradiation demanded by theory existed no clear vision would at all be possible. We showed that there was no sudden increase of scattering at the boundaries of the blind-spot and little *visible* scattering elsewhere. In fact Donders[7] was unable to detect any scattering around the edges of the blind-spot when he observed the optic disk through an ophthalmoscope when the image was 1/20 the diameter of the disk, while here we have worked with a ratio that was far greater, 1/200!

Another way of approaching the problem of scattering and blur circles is to calculate them, measure the amount present in actual observation, and then see if there is sufficient to account for our results. Volkmann[8] found that threads 0.0445 mm. were indistinguishable as two at such distance that the blur circles must have been 0.0035 mm. The image on the retina was 0.0019 mm. and hence the total image on the retina could have been not more than 0.0054 mm. Pope,[9] another investigator working with Helmholtz, found the irradiation circles to be 0.015 mm. *by experiment*. Listing[10] has given 0.022 mm. as the value of the diameter of a blur circle of a point 3 meters away from the eye, assuming the pupil to be 4 mm. wide. Using a somewhat different formula I have calculated the size of the blur circles from a point at 3-4 meters and find the value given by Listing to include the *third* maximum band.[11] If we take the first maximum to be 1, then the following bands have the following values:[12] $2 = 0.01745$; $3 = 0.00415$; $4 = 0.00165$; $5 = 0.00078$. It is very evident that the weakest stimulus seen in the blind-spot would not give rise to a noticeable sensation beyond the *third* band. Taking the value of 0.022 mm. as the limit of visibility of scattered light due to blur circles or diffraction bands and adding it to the size of the image of a 2 mm. stimulus at 4 meters, 0.005, we get the value of 0.027 mm. as the total effective image on the retina. Since the average blind-spot is 1.50 mm. we find we have a nearly 60-fold margin of safety before the edge of the blind-spot is reached. Here both theory and fact agree: the small amount of scattering is attested by observations in which it was reported that the phenomena seen do not extend to the boundaries of the blind-spot with the smallest and weakest stimuli used.

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[7] Donders, cited by Helmholtz, *op. cit.*, II, 27.

[8] A. W. Volkmann, cited by Helmholtz, *op. cit.*, I, 121 ff.

[9] B. A. Pope, Beiträge zur Optik des Auges, *Arch. f. Ophth.*, 9, 1863, 60-64.

[10] Listing, cited by Helmholtz, *op. cit.*, I, 132.

[11] In my calculation the value of the longest visible wave-length was used because the diffraction bands are greater for the red end of the spectrum than for the violet. The formula used for the size of the diffraction bands, assuming a point source and that the pupil is a circular aperture, is as follows: $\frac{1}{2} X \doteq d\frac{M\lambda.1.2}{W}$ where X is the total diameter of the diffraction band; d is 15.5 mm. or the distance from the nodal point of the eye to the retina; λ is the dominant wave-length of the source; M is the desired maximum (for 1st, $M = 1$, 2nd, $M = 2$, etc.); W is the width of the pupil and 1.2 is a constant used in the treatment of circular apertures. It will be seen that in order to get the diameter of the diffraction bands, *i.e.*, the distance on either side of the center, we must double the right-hand member of the equation, hence the use of ½ X in the left-hand member.

That there is diffusion or scattering through the optic nerve head we do not deny. The nature of the phenomena observed in the blind-spot show it to be true. Since the diameter of the cones is given as from 0.002 to 0.004 mm. our 2 mm. stimulus at a distance of 4 meters would be large enough to affect only one and at the most two cones. Since a stimulus of this size does not appear as a point even in direct vision we should not expect it to in the blind-spot. But the amount of haze is so much greater in the blind-spot than elsewhere that one is inclined to ascribe it to the fact that end-organs are lacking and the optic nerve fibers act as refracting substances which tend to scatter the light within the optic nerve head.

We have therefore three possible ways of explaining our results.

(A) What is seen is due to adjoining retinal elements. The following facts, in summary, argue against this conclusion:

(*1*) Some form perception exists in the blind-spot, vague, yet present.

(*2*) The least perceptible color seen in it is different from the faintest color seen on the edges.

(*3*) Two colors may be seen simultaneously within the blind-spot.

(*4*) When an annulus of color surrounds the blind-spot and another is exposed inside, neither affects the other, often a black space between is visible.

(*5*) Stimuli nearest the edges of the blind-spot are not the most effective in the blind-spot.

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[12] Taken from E. Verdet's *Leçons d'optique physique*, 1869, I, 306.

(*6*) Apparent movement may be seen in the blind-spot under conditions highly unfavorable to its perception outside.

(*7*) When the light seen in the blind-spot does not extend to the edges we should have to assume that subliminal stimuli outside the blind-spot can give rise to supraliminal effects inside.

(*8*) Characteristic peculiarities are found in the blind-spot which do not hold for other parts of the eye: peculiar changes in color with prolonged observation, rapid adaptation, and the like.

(*9*) Light does not take on the form of the blind-spot when only adjacent parts of the retina are illuminated while the blind-spot is kept dark.

(*10*) When the conditions demanded by the scattering and irradiation theories are fulfilled characteristic phenomena observed in the blind-spot are lacking which are present during direct stimulation.

(B) The second hypothesis is that there may be end-organs in the optic nerve head which are responsible for the phenomena seen. The fact that several cores are seen when the stimulus has several centers of brightness or several stimuli are exposed simultaneously may argue in this direction. If such organs exist it is my opinion that they must be rudimentary and probably do not number over a few score at the most. It is to be hoped that histologists will investigate again this region of the eye since little is actually known concerning this point.

(C) The third hypothesis, that the optic nerve fibers are responding directly to light, fits in with a rapidly growing belief that all protoplasm is affected by light, that the phenomena of excitation and transmission of nerve impulses are electrical in nature, and should receive serious consideration. If it is remembered that definite receptors for smell, thermal, and the numerous cutaneous qualities have never been found the rod-cone hypothesis will not seem so sacrosanct.

But since we would expect the regions supplied by the nerve fibres to be seen if they respond to direct stimulation by light in accordance with the well known principle of eccentric projection or constancy of place coefficients, it seems hard to believe that the fibres themselves are responsible for vision with the optic disk. We have pointed out above that this same objection applies to the hypothesis that adjacent retinal elements are responsible for what is observed in the blind-spot, because by the same token the stimulation should be referred to the places actually responsive to light and not to the optic disk. An objection which is equally fatal to the theories which do not allow the optic disk the functional rôle we have claimed for it does not help us in finding a positive explanation for vision there, even though it helps dispose of irradiation and allied hypotheses. We therefore find ourselves favoring the second of our hypotheses (B) which does provide the necessary mechanism in the optic disk without being open to the charge of violating the accepted principle of eccentric projection. If, however, stimulation along nerve trunks is not always referred to the regions supplied by the ends of the fibers, we should favor the (C) hypothesis.

GLOSSARY

Annulus, ring.

Blind-spot, supposedly blind area practically coincident with the optic disk.

Blur Circle, blurred vision of an edge or point caused by imperfections in the optical system of the normal eye.

Central Core, Example: When a light stimulus is applied to the blind-spot the resulting effect resembles the appearance of a light from a head-light showing through a thin fog or haze; the central core is the center of the illuminated area and stands out because of its greater intensity and definiteness.

Chroma, saturation of a color; "richness" of a color.

Chromatic Aberration, unequal bending of light rays of different colors due to imperfections in the optical system of the normal eye.

Cortical Projection of the Retina, region of the cortex of the brain reached by nerve fibers from the retina; region of the cortex most intimately connected with the retina.

Diffraction, the bending of light rays due to their passing an opaque edge.

Diffraction Band, a series of light and dark bands due to diffraction.

Diffusion, dispersion of light rays in several directions from a given point.

Eccentric Projection, localization of an object in space away from the observer.

Einprägung, impression; stimulation.

Filters (optical), glass or gelatine plates used for the purpose of reducing the brightness or altering the color of light.

Gamma Movement, apparent expansion or contraction of a stimulus-object when the degree of its illumination is suddenly changed as when turning the illumination on or off.

Halo (as used), ring of diffuse light surrounding the central core.

Infraliminal, below the limen; too weak or too small to be perceived.

Irradiation Circle, space of lessening activity around an image (on the retina) due to the lack of abruptness in functioning between stimulated and unstimulated areas.

Liminal, value of a given stimulus just below which the stimulus is imperceptible and above which it is perceptible; pertains also to just perceptible differences.

Macula Cæca, pit in the center of the retina, in the region of clearest vision (fovea centralis). Used here to mean center of blind-spot.

Meridians (as used), imaginary lines passing along the curved surface of the retina from the fovea to the periphery (outer edge).

Negative After-image, after-effect of stimulation of the retina in which the qualitative opposite (complementary) of the brightness or color of the stimulus is seen; example: green after-image of red.

O, observer.

Optic Disk, region where fibers of the retina converge to form the optic nerve; optic papilla.

Optic Papilla, optic disk; entrance of optic nerve into the retina.

Positive After-image, momentary persistence of the stimulus-effect after the removal of the stimulus.

Projection of the Blind-spot, area of the visual field the reflected light from which falls upon the blind-spot; this area increases in size with distance from the blind-spot.

Supraliminal, above the limen; intense or great enough to be perceived.

Tint, brightness of a color; light as opposed to dark color.

READING TWENTY-THREE[1]

PHOTOGRAPHING ARTISTIC VOICES[2]

Editor's Note

When the figure-ground concept is used in interpreting the facts of sensory processes it is not necessary, nor is it correct, to suppose that the ground must be confined to the same sense department in which the sensation is occurring. Auditory qualities often stand out upon a visual, kinæsthetic ground. When tones are first heard by the infant the character of the ground on which they depend would probably be more evident, if an adult could observe it, than the corresponding ground in adult experience, for the reason that these qualities, in the adult, have become so highly differentiated. Nevertheless, it is obvious that the pitch of a tone depends upon perceiving it with reference to a *scale,* a total pattern which in part gives to the particular tone its property of pitch just as a scale of related numbers, say from 1 to 10, gives to the apprehension of the number 8 the property of "eightness." In part the pitch is determined by the stimulus, but tones do not possess the attribute of pitch for a person who lacks the auditory insight of hearing tones with respect to a total range. This is the difficulty with so-called "monotones." The external stimulus is, therefore, only *one* essential condition out of many.

Then, of course, melody and chords are examples *par excellence* of auditory configurations. The artistic qualities of the singing and speaking voice are also good examples.

Every one knows that he likes or dislikes certain singing and speaking voices but he does not know what causes his likes and dislikes. He hears the voice as a whole, unanalytically, and reacts to it as a whole. Many of the conditions for beauty of tone have long been mysteries. Professor Metfessel has reduced the speaking and singing voices to minute photographic analyses, exposing many of the hitherto unknown conditions necessary for their artistic quality. It is evident, from Professor Metfessel's discussion, that artistry of voice is a configuration for which a vast number of conditions are responsible.

If you should pick up a Victor or Columbia phonograph record and examine carefully the grooves making the track for the reproducing needle, you would see what might be called a picture of speech or music. In those grooves you find a representation of everything that makes up speech or music physically.

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[1] Parallels *The Science of Psychology,* Chapter XIII.

[2] By Milton Metfessel, University of Southern California. Written specially for *Readings in Psychology.*

If the phonograph record was made at the ordinary rate (78 revolutions per minute), and you should have a record of a tone on middle C, under a microscope you could count nearly 200 tiny waves in one circumference.

Each wave has a certain width and design. If the tone is loud, the chances are that it will occupy more space laterally on the record than if it is a soft tone. If the tone is rich, the chances are that it will have a more complicated, or elaborate design than if it is a simple or "pure tone," such as that from a tuning fork. Of course, for a tone on high C there will be twice as many waves as for middle C, and for low C, half as many; the higher the pitch, the more waves per second, and consequently the shorter the distance of a wave along the circumference of the record.

The groove in a phonograph record does not lend itself readily to measurement and study for a number of reasons. The groove itself is so wide in relation to the width of the wave itself, that it makes the waves' patterns rounded, increasing the difficulty of locating the beginning and end of a single wave exactly. The waves on phonograph records are obviously too small both in width and length, and a spiral is not so satisfactory as a straight line for measurement.

Experimental procedure in phonophotography. A number of devices have been constructed for amplifying the phonograph record waves on smoked paper, making them of sufficient size for measurement, notably those of Scripture and Merry. Others have been invented for enlarging the waves on motion picture film. The devices for photographing and magnifying the waves from phonograph records can just as well be used directly from the voice; but for purposes of our discussions we will limit the technique to phonograph records. It is artistic voices which here will chiefly concern us, and the world's celebrated concert, operatic, and dramatic artists have records of their voices available to any laboratory for photographic analysis.

In order to give a general notion as to the way sound vibrations may be transformed into light vibrations, in putting the waves from record to film, we will consider one of the simplest devices, called the *Dorsey phonelescope.*[3] The phonograph record is played, and its sound waves are directed against a diaphragm in the phonelescope, such as is found in a phonograph reproducer. Near the center of the phonelescope diaphragm there is attached a tiny spring hook which extends out about half an inch. A fine strand of silk thread runs from the tip of the hook back to the diaphragm, wrapping about a tiny axle about half way from hook tip to diaphragm. The axle moves in jeweled bearings such as are found in watches, and if the diaphragm moves, the thread conveys the motion to the axle. On the axle is mounted a mirror.

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[3] Dorsey, H. C. "Phonelescope," *J. Opt. Soc. Amer.*, 1922, 22, 473-479.

A light is reflected from the mirror and concentrated by a lens to a tiny point on motion picture film. When the mirror on the axle moves, the light point on the film moves with it. The motion of the system thus starts with the needle in the groove, and is carried to the reproducing diaphragm, thence to the sound waves in air, the phonelescope diaphragm, thread, axle, mirror, and the light point on the film.

An example of waves photographed by this process is found in Fig. 28. The simple sine wave from a 100 dv. tuning fork, is used for the timing. The other wave is from the voice. It might just as well be a groove in a phonograph record as far as its general pattern is concerned; to be sure the needle would not make such wide or sharp curves.

The four properties of vocal tones. There are four properties of voice waves which we may study from such photographs and in terms of these four we will in time be able to describe any vocal characteristic. With only the four variables we can describe the difference between an artistic and a near artistic voice, a Caruso and an amateur, vocal emotional expressions, primitive or folk music characteristics, and language or dialects.[4] They are:

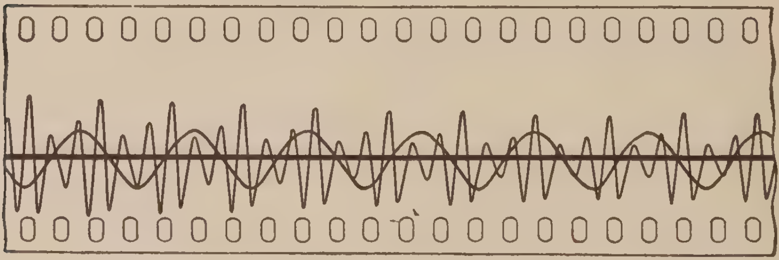

FIGURE 28 (Original FIG. 2). Voice wave and timing wave (sine curve). (After Metfessel, 1926.)

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[4] Seashore, C. E., "Phonophotography in the Measurement of the Expression of Emotion in Music and Speech." *Scient. Mo.*, 1927, 24, 463-471.

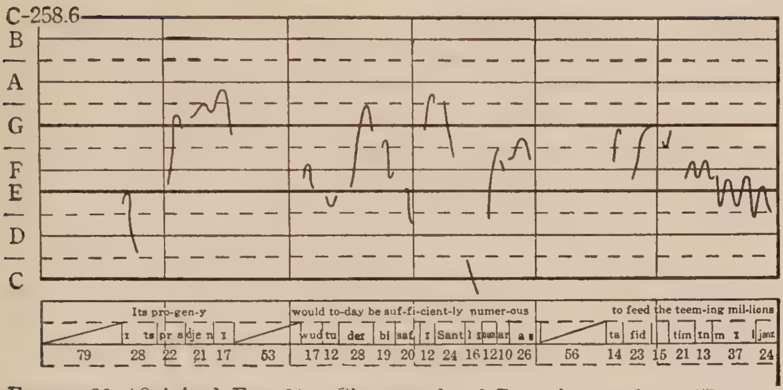

FIGURE 29 (Original FIG. 9). Six seconds of Bryan's speech on "Immortality." (After Metfessel, *Phonophotography in Folk Music*, University of North Carolina Press, 1928.)

1. Wave frequency, which is the number of waves or vocal cord vibrations per second. The experiential correlate is *pitch*.
2. Wave height, which is the amplitude of vibration, or the distance from the center of the wave to the highest peak. The taller the wave, roughly, the more intense the voice sounds.
3. Wave form, or the design of the wave. Wave form's psychological correlate is timbre, or quality. If you have two different wave forms from the same voice, you may have two different vowels, or two different emotions expressed, or other differences in quality.
4. Wave recurrence, or the duration of any voice wave train you may care to isolate. In vocal studies the duration of a sound or syllable is often significant. Out of this property and wave height, we compound vocal rhythm.

Even by boiling down the factors to be studied to four, it is a gigantic task to describe any single voice characteristic fully in terms of even one of them. At the present, voice experimenters who are interested in psychological problems are inclined to concentrate upon the two properties of wave frequency (pitch) and wave recurrence (time), because they are simplest and basic.

Speech melody. For instance, all problems of melody are concerned with pitch and time. In singing, where the tones are prolonged, we have a much simpler situation for the determination of melody than in speaking where the tones are rarely prolonged. To put speech on a musical scale satisfactorily requires a refined photographic analysis.

In ordinary conversation we are not aware of any particular relationship of the voice and a musical scale. When a person becomes frightened, we may note that his voice becomes strained and high-pitched, or when tender his tones may be husky and low. But aside from such quite general considerations, we pay little attention to the way the voice glides about the scale, or momentarily perches on a note. There is no sharping or flatting in speaking. All this is natural because speech melody is perceived mostly as meaning. If we say ''oh'' with a rising voice, we mean quite a different thing than if we say ''oh'' with a falling voice. Some languages, African for example, are called ''tone'' languages because the same syllable when spoken on a high tone (not a glide upward) means something quite different than when spoken on a low tone. Missionaries who have not mastered this difference have been chagrined to find that the words they used were interpreted with profane rather than sacred meanings.

Speakers of English often use a musical scale, but its use is for speaking effectiveness by making their voices sound more musical, rather than for meaning. Bryan used a scale close to the even tempered, provided we refer to that part of his speech where his voice holds a tone for a tenth of a second or more. Such an effect is observable in a graph of Bryan's voice in ''pattern notation.'' (Fig. 29.)

Pattern notation. After we have the sound waves transferred from a phonograph record (in this case Bryan's record on ''Immortality,'' Victor 1616-b) to the motion picture film, we then have to measure the waves before we can graph the melody of speech. There are many possible ways of measuring the waves depending upon the degree of fineness desired. For this particular record, we counted the number of waves and fractions of a wave (in tenths) occurring in $1/100$ sec. Later we found that one-fiftieth of a second would have been a more satisfactory unit, taking less time and giving a smoother curve; for in a study of speech melody the slight jogs in the voice are not needed, however important they may be when made objects of study in themselves.

To proceed with the method of measuring Bryan's voice so as to plot its tonal variations on pattern notation: knowing the number of waves (and fractions) in every $1/100$ sec. of the entire record, we only had to multiply each of those values by 100 in order to convert it into the standard numerical unit of frequency of vibration, or the number of vibrations per second. If in $\frac{1}{100}$ sec. there are 1.7 waves, the frequency of vibration is 170 dv. (double or complete vibrations). "Reading" the Bryan film of some five hundred feet was a matter of compiling lists of figures in dv. for each $\frac{1}{100}$ sec.

Turning to the framework of the pattern notation, it will be noticed that the five-line staff in common use in the conventional notation did not meet our needs, since we desired an equal amount of space upward on our graphs to represent a half-step, just as we used an equal amount of space from left to right to represent time in seconds. The letters (C, D, E, F, etc.) for the half-steps of the even tempered scale, with standard A having 435 dv., are found at the left of the pattern notation. The white keys on the piano are represented by unbroken lines, and the black keys by broken. Each letter has a definite numerical value in dv. (although not shown on the display pattern notations), so that from these half-step points on the scale we assigned points for 80, 90, 100, 110 and so on vibrations per second.

For time, we drew heavy vertical bars from top to bottom for every second, so that one graph of the pattern notation represented six seconds. We divided the seconds into tenths by the five dashes and five spaces of the broken lines.

We then graphed the figures compiled from the measurements of the film in $\frac{1}{100}$ sec. units. Starting at the left of the pattern notation, we placed dots in their respective frequency of vibration and successive time positions as in any graphic procedure. When the dots were connected with solid lines, we had the tonal course of Bryan's voice, as exemplified in Fig. 29. If you follow through the graph you will see that *It's* was spoken approximately on E, *pro-* on G, *-gen-* on G♯, *-y* a little above; then *would* strikes F, *to-* perhaps D♯, *-day* G♯, *be* slides from F♯ to F, and so on. To be sure the notes of speech are not specifically "on the pitch," but a study of the entire record showed that *there was a tendency for the notes to cluster around the scale half-step.* The notes are not pure, that is to say, each syllable generally has a glide of some kind which takes as much time as the note. *It's* is typical with a falling intonation of a whole step releasing the note.

The language of the pattern notation. In planning such graphic procedure, we wanted to make the notation so clear that it would tell its own story and convey meanings which would be difficult to put into any verbal description. We have found that studies of the way great voices look when their tonal movements have been put on pattern notation have opened up new auditory worlds for us. This is to be expected because there are many examples in psychology of the value of utilizing as many senses as possible in perceiving external objects and events. There are cues of voice characteristics in the visual picture which probably would not have been noticed in hearing alone, and even if noticed by ear, there would have been no satisfactory way of understanding them or measuring them without first measuring the sound wave.

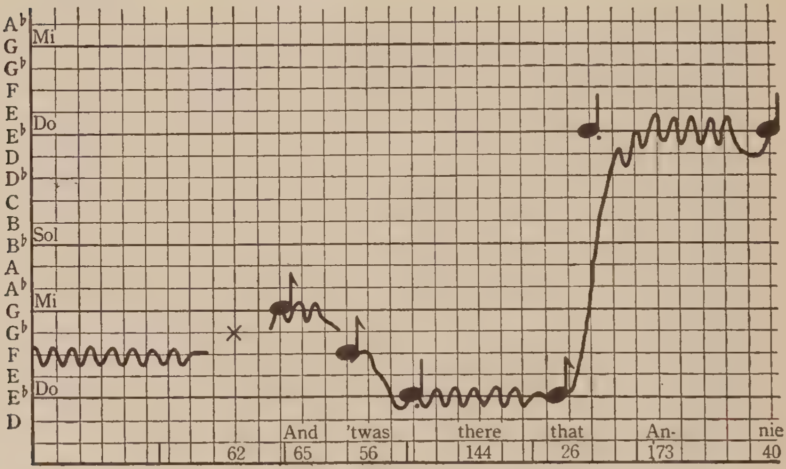

FIGURE 30 (Original FIG. 8A). Early pattern notation of the first part of McCormack's "Annie Laurie." Each graph represents six seconds. (After Metfessel, 1926.)

The vibrato in artistic singing. Of such a character was the vibrato. Take the way McCormack's tones graphed up (Fig. 30) on his rendition of "Annie Laurie." (Victor 846-A.) This is an older form of the pattern notation than is shown in Bryan's graph (Fig. 29). Every note has cyclic rises and falls on the scale, averaging nearly a half-step in extent and about seven cycles per second in rate. Until some of us saw this graph of McCormack's voice, we were blissfully ignorant that there was a vibrato there at all.

Flat denials have been made of a rumor that all great artists possess a vibrato in nearly every tone. However, when we photographed sample tones from some forty artists and long passages of ten of the most renowned, we were practically certain that the "rumor" was true; the vibrato was universal. In Fig. 31 seven artists are represented by the way they sang "And" of the twelfth measure of "Annie Laurie." By this time we had come to associate our auditory experience with the vibrato pictures (as shown on McCormack's graph), so that we felt sufficiently fortified to play record after record to hear if the vibrato ever completely passed out of the voice of an artistic singer. When there was any doubt we slowed the

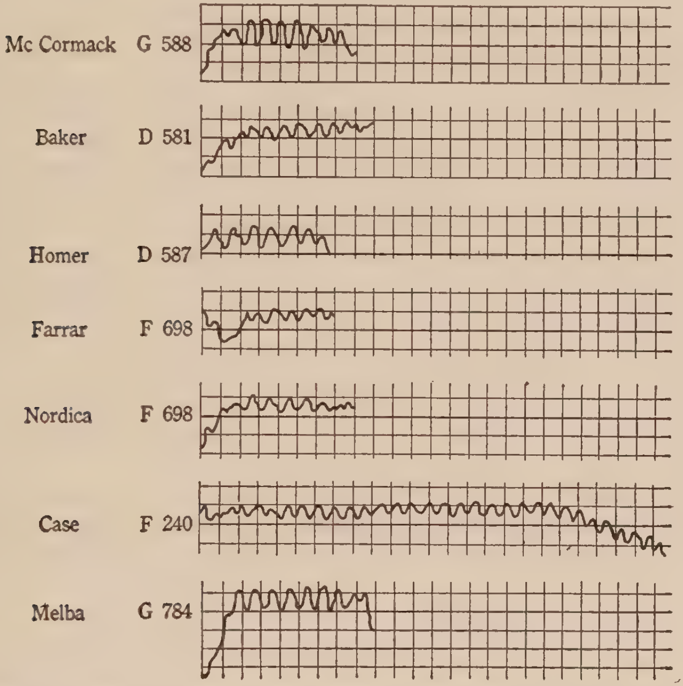

FIGURE 31 (Orig. FIG. 9). Seven artists on the same tone, "And" from the twelfth measure of "Annie Laurie," all showing a vibrato in their voices. (After Metfessel, "Technique for Objective Studies of Vocal Art." *Psychol. Monog.*, Vol. 36, 1926.)

record down, because the vibrato becomes more noticeable the slower the record is played. Our conclusion was that every artist has a vibrato, and that as a group artists use the vibrato in 95 per cent. of their tones. Even the short notes have one or two pulses, or cycles. Little did we realize what a stir this would make from a few who only had their ears as an instrument for detecting the vibrato. All of which goes to show that what can be seen on the pattern notation suggests characteristics to be listened for in an actual musical situation. No one could miss seeing the vibrato on the graphs, but it is often missed by ear.

The vibrato (sometimes called tremolo, unfortunately) has been a source of great perplexity to musicians. Voice teachers do not know whether to give any attention to it in teaching or not, unless it is offensive; or they find upon going to the literature to find out anything about it, that they are confronted by an amazing mass of contradictions. The vibrato has been branded as inartistic and ugly by some, and as the champagne of the voice by others. Some have called the vibrato a variation in tone, and others in intensity (tone power), and still others refer to it as a variation of both. Such contradictory statements are not stupid, or the result of temperament, but can mostly be traced to the astounding differences between ears and in the way people train themselves to mentally organize the vibrato. Phonophotography has enabled us to settle quite definitely some points about the vibrato, which ear studies would have left on a folk lore level. The voice photographs are not blighted by individual differences, for two photographs of the Bryan or McCormack records will show the same measurable results, whereas two ears will not. Not at all that auditory experience on a sensory level is not predictable. We cannot leave hearing out of a scientific account of voice characteristics, but the measurements of what is heard needs must be based on the auditory stimuli if they are to mean anything to voice scientists.

An example of the application of Gestalt psychology to vocal æsthetics. A neat example of the use of *Gestalt* concepts in explaining considerations of the beautiful has appeared in our vibrato studies. To explain it, we will return to McCormack's graph where we saw the vibrato appearing as a cyclic rise and fall on the scale. When this effect is heard by a musical layman, he does not hear a rise and fall of pitch, but one and only one, steady salient pitch.[5] It is located intermediately between the extremes. *Here the vibrato tone is perceived as a whole, and the mental organization of it in no way resembles its physical structure.*

It is possible to factor out the upper and lower pitch limits of the vibrato if one so trains himself, just as the overtones of a violin can be singled out by ear. But if you listen for the upper pitch limit of the vibrato, the minute you tune it in, the whole tonal complex changes. What was a rich, thrilling beautiful tone becomes in an instant thin, spattering and ugly: all done by a simple "twist of the mind." This is exactly what the *Gestalt* psychologists have been saying all along; if you analyze you have an entirely different piece of mental furniture. The reason many musicians and critics come to dislike the vibrato is that they are continually analyzing it, while the layman in the audience is demanding his opera with it or not at all, because what he hears is quite different from what the analyzing musicians hear.

You can verify these statements experimentally if you select a phonograph record of some celebrated artist, and listen carefully to a tone with little orchestral accompaniment. After you have located the tone, stop the record at its beginning. Then allow the record to gather its own momentum. You will hear the two notes of the vibrato climb after one another up the scale, until the alternations become between four and five per second. From there on up there is a single pitch which sounds as though it were beating. By turning the speed indicator, the record can be played at, say, 40 revolutions per minute, where the rise and fall of the pitch in the vibrato is plainly evident.

In defining the vibrato, should it be referred to as rise and fall of pitch, or as having no pitch fluctuation? Obviously, if you define from the analytical standpoint it has two pitches, but when so perceived it is ugly; and if you define it as the layman hears it, it has no pitch fluctuation. The definition should include the situation under which the vibrato is studied, or it will rightly be wondered how you can say the vibrato has both one pitch and two pitch limits.

Norms of beautiful vibratos. The vibrato likewise may be used as an example of the value voice photographs may be to the voice student or his teacher. The measures can prescribe in unmistakable terms the way artists use their voices. We have made measures of a sufficient number of artists and selections to state quite definitely the rate, extent, and form characteristics of the vibrato. Students may compare their vibratos with these norms.

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[5] Metfessel, M., "The Vibrato in Celebrated Voices." *Sci. Mo.*, March, 1929.

The significant thing about the norms of the artistic vibrato is that artists resemble one another strikingly in the various factors. When the composite distribution of any one factor for all the artists is compared with any single one of them, the difference is relatively slight. Not so with the voice students, whose vibrato characteristics may be described as hanging around the fringes of the artistic norms. As well as being consistently alike, the vibratos possessed by artists have a rare combination of factors, so that if a voice student has the same rate as artists, he may not have the same extent, or *vice versa*. There are quite definite boundary lines which set off beautiful vibratos from the mediocre and impossible.

The average artistic rate is close to seven cycles per second.[6] No artist that we have studied ever averaged less than six and a half, although some music students have an average of less than six. The variability of rate with artistic vibratos is about a pulse and a half, one each side of the average of seven; although these extremes are rare, with most rates hovering around the average of seven. Voice students as a whole lack this flexibility, especially in the faster rates.

The average extent of the artistic vibrato is a half-step, varying from one tenth to over a whole step. In general, voice students only average a third of a step, and rarely go higher than two thirds of a step as their vibrato pitch limits; again being more restricted than the artists. The discovery that the vibrato was not as wide in pitch extent among voice students as with artists seems odd, because one's first reaction to many voice students is that there is too much vibrato there. It is not the extent on the scale, though, which bothers us, but rather the variations in intensity and timbre that go along with the pitch (speaking analytically). The artist has mastered the trick of using a wide pitch vibrato, which is not objectionable (our synthetic vibrato devices have proven this) without at the same time increasing the intensity and timbre variations of his voice to painful extremes. When the average voice student attempts to increase the pitch extent of his vibrato, the intensity and timbre limits are likewise increased. As for vibrato form, a statement of norms for it involves a more extended treatment than would be advisable to give here.

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[6] Metfessel, M., "What Is the Voice Vibrato?" *Psychol. Monog.*, 1928, 39, No. 2, 126-134.

Fast fluctuations in good speaking voices. One of the æsthetic advantages of the vibrato is that it makes a singing tone appear sparkling and full of life. A tone without vibrato, such as a tuning fork, cannot be used in solo situations because its beauty soon palls. The vibrato introduces an element of variety, since it is never quite the same in either its rate, extent or form from one tone to the next. It is continually playing between its limits, described above, enough so that it keeps the artist's tones from getting monotonous.

The brief, sliding tones of speech eliminate the vibrato as a possible universal vibrant effect in the speaking voice, but Miss Shimmick has found good speaking voices to be characterized with fluctuations from just faster than the vibrato rate to as high as fifty per second. Where the speech tones are prolonged, the vibrato may or may not appear. The artistic effect of the faster fluctuations is somewhat the same as the vibrato, although far more subtle; but leave them out and the speaking voice sounds flat and uninteresting.

Artistic vocal noise. Another cue that came from voice photographs suggesting a problem that the unaided ear might have missed (in part because auditory experience is transient whereas the photographs may be studied at leisure) was found in a picture of the word "Revenge!" as spoken by E. H. Sothern in the Shylock speech. (Victor No. xxxx.) Noise on the sound wave appears in the photograph as a jumble of vibrations, rather than a series of homogeneous waves such as one finds in a tone, and "Revenge!" was exactly that. We were trying to read the frequency of the waves, and were in difficulties trying to follow through a series of waves. Miss Cameron photographed a large number of such vocal noises, of which at least three kinds are a definite part of some dramatic artists' vocal repertoires. One kind is evidently from the vocal cords, another from the cavities above, and a third a combination of both.

Vocal emotional expression. The emotional coloring of a tone has something to do with pitch patterns of the voice, such as Merry's findings that intense emotions shoot the voice to either a high or low extreme instead of the neutral middle-pitched conversational tones, and in voice breaks where swift pitch changes have a characteristic part. It is common observation that strong emotions are often expressed by loud tones; and the timbre of the voice is different for whining than for gaiety. Noises likewise play a part in vocal emotional expression.

It soon became apparent that dramatic artists often expressed a disagreeable emotion with disagreeable tones. But the noise element is not overdone; rather it is skilfully maneuvered among rich tones. Sothern in his Shylock speech does not hammer away with a series of noises, but he suggests the disagreeableness of his anger by striking in a noisy tone on emphatic words now and then. These findings may have some bearing on the problem as to whether the dramatic artist feels the emotion he is expressing. It would seem that the actor who works his anger lines into such a nicety of balance between noise and rich tone is not in the same sort of emotional situation that an ordinary man is when angry. If the tones of the angry man are noisy, there would be no organization, or suggestiveness about them; no order in their appearance in a setting of rich tones such as can be traced in the actor's.

Phonophotography in Folk Music. Coincident with our studies of artistic vocal performances, we have used the phonophotographic technique in comparative studies of folk music. Up to the present time notations of folk music have been incomplete because the ear hears only gross effects, and thus cannot serve as a basis for symbolizing some of the quite distinctive patterns of the different folk musics. Using a portable phonophotographic camera, we made a study of American Negro folk music, going into the woods of North Carolina, and to Hampton Institute in Virginia for the songs and singers. One of the more primitive spirituals is exhibited on pattern notation in Fig. 32. A collection of these songs including spirituals, blues, work songs, corn field hollers, and the like has been put into monograph form by the writer.[7]

The gliding characteristics of Negro singing, so evident in Fig. 32, give it part of its mournful atmosphere. In the third second of graph two, the voice glides over an octave up to the note itself on G. The more formal spiritual, like "Swing Low, Sweet Chariot," looks somewhat like McCormack's "Annie Laurie," showing the similarity between formal spirituals and the European type of singing. The work song is characterized by a grunt, or "huh!" which is inserted when the workman's pick strikes the ground. The "huh!" generally has a circumflex form of varying duration and extent. As far as the melody of the song is concerned, the grunt is neutral; not at all fitting in but not interfering, although it can be assigned to a definite note. The blues frequently have a certain type of voice-break which we are calling the "falsetto-twist." In speaking during grief the voice often cracks, and the Negro has crystallized one form of this pattern into his weird melodies. It consists of a sudden and brief penetration into falsetto with just as quick a return to the natural register. On the graphs the falsetto-twist looks spindle-shaped. The corn field holler is much like a yodel, the voice changing from falsetto to natural register on various notes, although most notes are the former.

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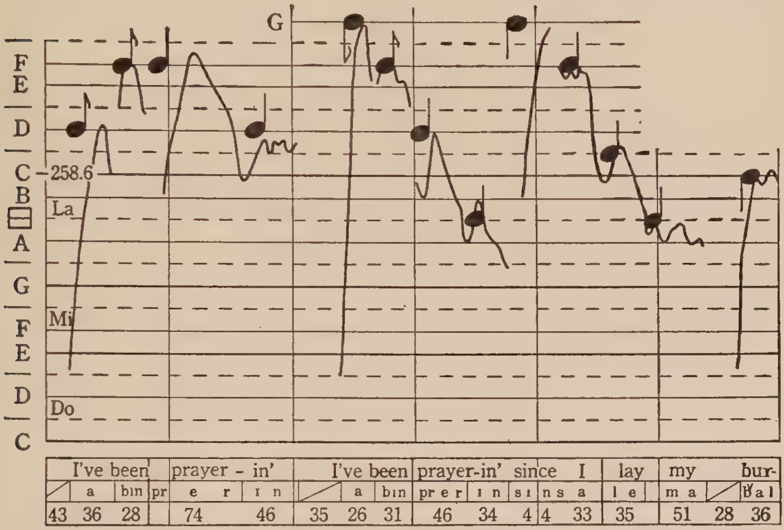
[7] *Phonophotography in Folk Music*, University of North Carolina Press, 1928.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35 26 31
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4 33
35
51 28 36

FIGURE 32 (Original FIG. 14). Pattern notation of a backwoods negro spiritual. (After Metfessel, *Phonophotography in Folk Music*, University of North Carolina Press, 1928.)

READING TWENTY-FOUR [1]

THE SENSORY ACTIVITIES OF THE SKIN FOR TOUCH AND TEMPERATURE [2]

Editor's Note

For many years it was thought that isolated nerve endings in all parts of the body were, as individual units, responsible for sensations, with the aid, of course, of the brain. Single rods and cones were supposed to give rise to visual sensations; single hair cells in the cochlea (the internal organ of hearing) were thought to elicit auditory sensations; and single cells of various shapes and structures were said to determine the cutaneous sensations, warmth, cold, pressure and pain. The fact of so much specialization of structure in these different receptors, together with their distribution, led to this belief. The possibility is only now being appreciated that perhaps the implied one to one correspondence between sense organ and sensation (constancy hypothesis) does not exist. The more plausible hypothesis seems to be that sensations depend upon sectors (areas of receptors plus their connections with the brain) whose *field properties* not only determine in part whether a particular receptor will respond at any one time, but also what the quality of the sensation, within any given sense department, will be. A particular alignment or balance of stresses between an area of the retina and the brain may be an important factor in determining whether the color seen is red, orange or yellow. Possibly the area on the retina, alone, possesses field properties upon which the responses of individual receptors depend. Similarly, a pattern or gradient of physiological activity in the skin may determine whether a temperature sensation is one of warmth or cold, or whether a tactual sensation is one of pressure or pain. It is by no means certain, therefore, that the same sense organs do not function in giving rise to more than one quality of sensation, depending upon their dynamic relationship to the affected area.

Such would be an attractive interpretation of the results obtained by Dr. Waterston and Professor Dallenbach, reported in this and the following readings. If the interpretation is correct, it would mean that configurational principles apply to the functioning of sense organs.

It is a well known physiological observation, that if the end of a slender metal rod, cooled or warmed to a suitable temperature, be applied lightly to the surface of the skin, a sensation of cold or of warmth is not produced at all points of the surface, but only when the point comes into contact with isolated areas, usually of small size. From the surface between these points a punctate cold or warm stimulus produces no sensation of temperature, and touch alone is felt. The responsive areas or points are known as cold and hot spots respectively.

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[1] Parallels *The Science of Psychology*, Chapter XIV.

[2] By David Waterston, M.D., St. Andrews University, Scotland. From *Reports of the St. Andrews Institute for Clinical Research*, Vol. II, Oxford University Press, 1924, 123-132. By permission.

Similarly, if the surface of the skin be carefully examined with a fine bristle, pressed lightly to elicit the sensation of touch, or with a firmer bristle, pressed more firmly so as to produce pain, the sensation of pain and of touch is not felt equally everywhere but is especially elicited at small spots termed touch and pain spots.

Based upon such observations, the current view of cutaneous sensation regards the surface of the skin as "a mosaic of tiny sensorial areas each sensitive to a specific stimulus, and the range of ascertained sensitiveness includes the sensation of touch, pain, cold, and warmth" (Sherrington (7)).

The discovery by Blix (1), corroborated independently by Goldscheider (3), of this punctate distribution of thermal sensibility was by these writers regarded as an illustration of Johannes Müller's law of the specific energy of nerve fibres applied to cutaneous sensation, and led to the belief in the existence in the skin and subcutaneous tissues of specific structural mechanisms, each subserving one or other form or sensibility, whether of touch, in the form of Pacinian and Meissner's corpuscles, or of cold or warmth, for which other forms of end-organs have been stated to act as receptors.

The theory of special end-organs implied also the existence of special nerve fibres connected with them, and the present view of cutaneous sensation postulates the existence of pain fibres, of touch fibres, and of warm and cold fibres, passing from the special end-organs to the central nervous system.

Sections have been made and examined microscopically of such spots or fragments of skin which were known to be sensitive to special stimuli, but such search has hitherto failed to demonstrate the presence at such spots of specific end-organs.

The cases of the loss of one or other of these forms of sensibility occasionally found in various regions of the body in syringomyelia and some other spinal conditions, such as a loss of temperature sensation, the loss of touch, or of pain, afforded apparent corroboration of the existence of these separate systems.

Head and Rivers's (4) observations upon the loss of sensation following division of a cutaneous nerve and during its restoration, showed that after such an injury all forms of sensation are not equally lost and are not uniformly restored. Their observations led Head and Rivers to put forward their theory of a dual mechanism of cutaneous sensation, the epicritic and the protopathic, each subserving certain forms of sensation and with special receptor organs and fibres for different forms of sensation.

They deny, however, the existence of a punctate mechanism for all forms of sensation, for in writing of temperature sensation they state that "in the normal skin the hot and cold spots are nothing more than minute areas peculiarly sensitive to either heat or cold, set in a territory over which temperature stimuli can also be appreciated."

The existence of special receptors and special nerve fibres for each of the classical varieties of cutaneous sensation, touch, pain, heat, and cold, cannot be considered to be proved, and indeed there are many features of sensation which throw doubt upon the existence of such a mechanism.

In a prolonged series of observations undertaken at the suggestion of Sir James Mackenzie, we have been able to confirm certain facts already known or suspected, and we have observed some others which throw considerable light upon the problem.

The methods employed have been described in the first volume of the *Reports of the Institute* (9).

TOUCH AND ITS RECEPTOR

Light touch can be excited from all portions of the surface of the body, as well as from certain mucous membranes, such as the conjunctiva and the buccal mucosa. As is well known, it is best elicited in a pure form by contact with a fine brush or wisp of cotton-wool, while on hair-containing parts the movement of a hair sufficient to cause motion of the portion embedded in the hair follicle gives the characteristic touch sensation.

Deformation of the surface is believed to be the stimulus effective for the production of the sensation. Examination by the binocular microscope of a portion of skin while it is being stimulated by a fine bristle, so as to produce the touch sensation, shows that the deformity necessary is of the very slightest character, and almost imperceptible. By such examination the surface of the epithelium is seen to have a somewhat unexpected character; it might be supposed that the surface would show a certain rigidity, but in response to a light touch it is seen to be very mobile, *e.g.*, over the tips of the fingers, and its surface behaves like an extremely thin membrane floating upon a fluid.

Differences of opinion exist as to the mechanism of touch receptors, but it is usually stated to be a function of specific "tactile corpuscles" and their nerves, while the inter-epithelial nerve fibres are stated to convey impulses which give pain sensation.

The terminal branches of superficial nerves end some in certain specific end-organs, while others pass to the epithelium and terminate among or even in the cells of the deeper layers.

It seems advisable to describe shortly the position of some of the common end-organs. There are, in man, probably no end-organs in the epithelium of the skin, the only form of end-organs which have been described as occurring in epithelium being the Merkel's "touch cells" in the snout of the pig.

These touch cells are large vesicular cells, probably epithelial in origin, lying in groups in the deeper layers of the epithelium, and with a network of neuro-fibrils in close relation to them.

Apart from these, the epithelium is richly provided with nerve fibres, which ramify and form a plexus in its deeper layers.

Nerve impulses are not readily produced by direct mechanical stimulation of nerve fibres; under ordinary conditions they are produced by the activity of receptor organs, and the arrangement in the skin suggests with the greatest probability that touch is not excited by direct mechanical stimulation of these fibrils by deformation of the surface, but that the epithelial cells are receptor organs, which are excited by contact and transmit to the nerve fibrils a stimulus which originates a nerve impulse in them.

The touch corpuscles of various types lie for the greater part in the subcutaneous tissues (Pacinian bodies), etc., or in the true skin; and of the latter group, Meissner's corpuscles lie most closely to the surface, occurring as they do in certain of the papillæ of the skin, while other papillæ contain capillary loops.

The distribution of such bodies shows conclusively that they cannot be the sole agents to act as receptors for touch.

Meissner's corpuscles are far too widely scattered in many regions to afford the necessary mechanism, for example on the volar surface of the forearm one such body was found by Meissner present in 35 square millimetres.

The Pacinian bodies are still less fitted to perform such a function, and their distribution suggests an entirely different use. They are found sparsely scattered in some superficial regions, and exist in considerable numbers in such tissues as periosteum, in the mesentery of the cat, etc., regions from which tactile sensation is entirely absent.

We examined the sensory activity of the exposed surface after removing different thicknesses of skin by blister. The pellicle of a blister is composed of a variable thickness of epithelium, including the stratum lucidum and more or less of the underlying layers. After a short application of a fly blister and in occupational blisters the pellicle may contain only the more superficial layers, but after prolonged application the greater part and even the whole of the epithelium may be removed.

When a distinct film of epithelium is left, the exposed surface is dry, and touch can be felt from it, but when more of the epithelium has been removed and the surface is raw and moist, no tactile sensation can be elicited, and pain only is excited by the stimulus.

The inference from the structural and from experimental evidence is that the epithelium of the skin is the receptor organ for the sensation of touch, performing the function of transmitting the stimulus excited by touch to the nerve terminals which are found among the cells of the deeper layers.

TEMPERATURE SENSATION

The activity of the cold and warm spots presents peculiar features, some of which have been from time to time recognized by various observers. The first peculiarity to present itself is the extreme difficulty found in enumerating the number of responsive spots present in a given area.

Goldscheider (3), who has been probably the most extensive writer on this subject, has pointed out that at a single sitting a true picture of the temperature spots is not obtained, but that the examination must be carried out for several days if all the spots are to be discovered.

Boring (2) states that it is one thing to map an area in a single investigation for cold or warm spots—it is quite another thing to find these spots a second time and not to find new ones.

The same difficulty was encountered by Trotter and Davies (8) in their observations upon cutaneous sensation in connexion with the division of nerves, and Head and Rivers (4) have drawn attention to the varying number of such responsive spots at different observations. Closely connected with this is the variability of situation of the spots, and their ready exhaustion by stimulation.

Our observations established the following facts:

(1) In different persons there is a very wide difference in the number of cold and warm spots which can be detected at any one observation. In an area of the same size (5 cm. by 2.5 cm.) it was found, for example, in one person that the number of cold spots which could be made out at a single examination varied from eight to eighteen, while in another the number was as great as sixty or seventy.

This difference in number was constant among the individuals, that is, in persons of one group the number was always small, in the other large, but could not be associated with any other condition. The relative numbers of the warm spots followed the same ratio.

(2) Repeated observations corroborate very completely the statement that one or two observations do not disclose the full number of spots within the area from which sensations of cold and of warmth can be elicited, and the sum total of the spots elicited by a number of investigations is vastly in excess of that found at any one time. In other words, under ordinary conditions, of the spots which are capable of giving the thermal sensation, only a fraction are active at one time.

(3) The spots are readily exhausted by a repetition of the stimulus. This is an important phenomenon, but of greater importance is the observation we have made, that dulling of one spot is accompanied by the awakening to thermal sensibility of an adjacent spot. If the point of a fine metal probe cooled to the requisite degree is brought into contact with a cold spot, and the touch repeated again and again at intervals of about a second, at first the contact produces a clear and distinct sensation of cold. After a few contacts, however, the sensation of cold is no longer excited by the stimulus, but that of touch only. The cold sensation may disappear after two or three contacts,

or only after some eleven or twelve, and it may disappear and then reappear once or twice and finally disappear.

If now the stimulus point be moved slightly to one or other side, the sensation of cold is at once set up from a spot from which before no such sensation was elicited.

THE PUNCTATE DISTRIBUTION AND "FLUCTUATION"

The explanation of this varying activity of the spots constituted a puzzling difficulty until we came to realize that we were dealing with a general character of the physiological activity of organs, namely, that under ordinary conditions the whole of an organ is not in a state of activity, but a small part of it only; that there is a coming and going of activity among the different parts—change from activity to quiescence. This feature we have termed "fluctuation," and recognition of its occurrence affords an explanation of many otherwise extremely puzzling phenomena. Some examples may be given:

(1) Fluctuation is very notable at the margins of areas of anæsthesia due to nerve and to spinal injury. In testing the extent of such an area it is a common experience to find that the margin of the area is not the same on two successive occasions—the margin is not fixed, but there is a zone of varying width at one time sensitive, it may be to all forms of stimulation, while at the next examination it may be insensitive. Perhaps more commonly the reverse is found, and the marginal area, formerly anæsthetic, is on a second examination sensitive.

(2) Such fluctuation is also found very notably at the margins of areas of hyperalgesia, whether cutaneous or deeper, and it is almost the rule to find the margin varying at different examinations. Recognition of such fluctuation in these areas was made by Sir James Mackenzie in 1883 (6), and the first record of it is as follows:

"Towards the end of January 1881 the patient became worse. He then exhibited the phenomena of hyperæsthetic bands encircling the abdomen, the hyperæsthetic portion on the left side being broader and a little wider than that of the right. The bands could not be distinctly mapped out owing to the variability of the impressions produced. Thus having enclosed, in ink, spaces where the patient declares he feels the pricking of a pin much less than natural, I proceed to other places. On returning to the first spaces I find from his starting and exclamation of pain that they have become hyperæsthetic. The dulled impressions are all either in the upper part of the hyperæsthetic area or slightly above, where they occasionally intermit with the natural about the level of the ninth rib on the left side and the tenth on the right.

"I have been unable to map out the areas on account of the extreme variability of the impressions."

(3) Sensory change and capillary change. The changes in the sensory activity of the "spots" suggested to us that there might be an associated variation in the capillary field. Evidence of the occurrence of such variation is easily obtainable in the condition, common among young adults, in which the fingers and hand are of a purple colour, made up of areas of a deep purple, and of smaller areas of a bright pink colour. If the skin in this condition is carefully watched, it will be found that the margins of the areas are constantly changing, the purple areas encroaching upon the pink ones, while the pink areas die away and new ones appear elsewhere.

Under the binocular microscope the purple areas are seen to owe their colour to a network of small venules filled with blood of purplish colour, and few capillaries can be seen. As the colour alters, small bright red capillary loops come into view in places where none were previously visible.

Examination of an area of normal skin showed that the number and arrangement of the capillaries visible in a given area alter with surprising rapidity.

Capillaries visible at one time disappear from view, and other capillaries make their appearance in the space of a few seconds. The occurrence of such capillary "fluctuation" so similar to the fluctuation in the sensory activity of the normal skin, suggests with considerable probability that the two phenomena are causally connected, but of this we have not yet obtained definite proof, as the technical details of the experiments necessary to determine it are extremely complex.

(4) The sensation of itching or tickling shows the same peculiarity. If the surface of the skin on the hairless part, for example on the front of the wrist, is gently rubbed with a bristle, the sensation of tickling is elicited distinctly from some parts and not from others. If one of these sensitive areas is gently rubbed the tickling sensation gradually disappears, but if the bristle is moved to an adjacent area, usually distal, the tickling sensation is again excited in an accentuated degree.

This gives an explanation of why itching tends to spread by scratching, for the scratching excites the sensitive part and its sensitiveness becomes exhausted, while that of an adjacent area is in turn excited.

(5) Alteration of the vascular conditions of the skin produces an extreme modification of its sensitiveness to thermal stimulation. We repeatedly observed the effects produced by applying a mustard leaf so as to cause an erythema on the sensory capacity of the part, and we found its sensory activity for heat and cold greatly altered. Such a reddened area is not hypersensitive to touch with a neutral body; light touch with the point of a pencil for example does not cause pain, but the area is extremely sensitive to warmth. Mild radiant heat, pleasantly warm on unaltered parts of the surface, causes at once a severe pain in the reddened areas.

The first effect of a mild erythema is to alter the sensation resulting from stimulation of the hot spots.

A mildly warm point applied to them gives no sensation of warmth, but, instead, one of sharp pain.

In a more pronounced erythema a further change occurs, and, if such an area is carefully examined for warm spots, a central area is found in which every touch of a warm point produces pain, and around this area is a zone of variable size in which, from practically every point of the surface, contact with a warm point causes a sensation of warmth. The sensation is not produced only from widely separated points but from the general surface.

The effect of the erythema is to disturb the "punctate" arrangement of warm spots, and instead of discrete and separated sensitive points, all parts of the surface may become sensitive for warmth.

The principle of "fluctuation" affords an explanation of this phenomenon also. The erythema induced by the mustard leaf has excited to activity all the sensory areas, and all of them respond to the warm stimulus. The effect of the mustard leaf is a "stimulation"—a calling into activity of all parts of the organ stimulated which are capable of activity.

SUMMARY

The principal results which have been reached include the following:

(1) There is structural and experimental evidence to show that the epithelium of the skin and of certain mucous membranes is the receptor organ for tactile sensation.

It is not merely a protective covering, but the receptor organ for a special form of sensation, *viz.* touch.

(2) At single examinations hot and cold spots show a punctate arrangement; the number of such spots in an area of given size, however, differs widely in different individuals.

(3) On repeated examination of the same area, the number of the hot and cold spots is greatly increased, new spots appearing on successive examinations, while many of those formerly found no longer respond.

These observations harmonize with the results obtained by practically all those who have investigated this subject.

(4) In addition, we find that under certain conditions, such as that of mild erythema, the sensation of heat can be obtained from practically all points of the affected surface at a single examination.

It follows from this that all parts of the surface are potentially sensitive to heat, and that there is no structural punctate arrangement of special receptors and special nerve fibres for thermal sensation.

(5) The punctate distribution of sensitive spots found at a single examination is explained by the view that in the skin there is a constant coming and going of activity of the different parts. Other examples of "fluctuation" are afforded by the skin capillaries in normal and in certain abnormal conditions, and the same principle may form the explanation of the variations found in areas of hyperæsthesia and anæsthesia.

To this coming and going of activity we have applied the term "fluctuation."

(6) The erythema produced by the mustard leaf acts upon the heat-receptive areas in such a way as to abolish temporarily this fluctuation and excites all of them to receptivity.

We do not discuss the bearing of these observations upon Head and Rivers's theory of a dual mechanism of sensation, protopathic and epicritic, each subserving various forms of sensation, but, in our view, the different forms of sensation obtained by these observers indicate differences in the physiological activity of a single mechanism rather than the existence of a dual structural apparatus, each of which is composed of different elements.

REFERENCES

[The fluctuation which is described here in the skin capillaries is amply corroborated by the observations of Krogh (5) which came under my notice after our observations had been made and this paper written.

The writer has pleasure in acknowledging the assistance contributed by the Medical Research Council to the St. Andrews Institute, which enabled the work upon which the foregoing paper is based to be carried out.]

(1) Blix: *Zeitschr. f. Biolog.*, 1884, 20, 141; 1885, 21, 145.
(2) Boring: *Quart. Jour. of Exper. Phys.*, 1916, 10, 1.
(3) Goldscheider: *Gesammelte Abhandlungen,* Leipzig, 1898.
(4) Head and Rivers: *Studies in Neurology,* vols. i and ii, London, 1920.
(5) Krogh: *Anatomy and Physiology of Capillaries,* 1923.
(6) Mackenzie: *The Lancet,* 1883, 1, 995.
(7) Sherrington: Schäfer's *Textbook of Physiology,* 1898, vol. ii.
(8) Trotter and Davies: *Jour. of Physiol.,* 1909, 28, 134.
(9) *Reports of the St. Andrews Institute for Clinical Research,* vol. i, 184.

GLOSSARY

Buccal Mucosa, mucous linings of the mouth.
Capillaries, minute blood vessels.
Capillary Loops, loops formed by minute blood vessels.
Conjunctiva, mucous membrane of the eye, over the cornea and under the eyelids.
Corpuscle, small body; in this case a nerve cell.
End-organs, sense organs; receptors; nerve-endings.
Epicritic Cutaneous System, differentiated system of cutaneous sense organs and nerve fibers responsible for differentiated and localized cutaneous experiences, and the last to return with nerve regeneration after an injury.
Epithelium, cells covering skin and mucous surfaces.
Erythema, redness (inflammation) of the skin.
Hair Follicle, depression containing the root of a hair.
Hyperæsthetic, super-sensitivity in general.
Hyperalgesia, super-sensitivity to pain.
Inter-epithelial, between epithelial cells.
Law of Specific Nerve Energy, the law (now questioned) that a particular nerve fiber, carried only one type of impulse no matter how stimulated. Example: No matter how the retina is stimulated (by light, jarring, application of an electric current) the resulting effect is always the perception of light, if the retina responds at all.
Mesentery, membrane supporting the small intestine.
Neuro-fibrils, fibers within a neuron.
Papillæ, small conic eminences.
Pellicle, thin membrane; film.
Periosteum, covering of the bone.
Plexus, network.

PROTOPATHIC CUTANEOUS SYSTEM, a relatively undifferentiated system of cutaneous sense organs and nerve fibers, responsible for undifferentiated, unlocalized experiences and the first to return with nerve regeneration.

PUNCTATE, pointed.

RAMIFY, to branch out.

RECEPTORS, end-organs; sense organs; nerve-endings.

STRATUM LUCIDUM, an underlying transparent layer of the skin.

SYRINGOMYELIA, distended condition of canal in the spinal cord. This canal is filled with liquid (cerebro spinal fluid).

VENULES, small veins.

READING TWENTY-FIVE[1]

THE TEMPERATURE SPOTS AND END-ORGANS[2]

Editor's Note

Professor Dallenbach's investigation corroborates in a general way the results obtained by Dr. Waterston. It would seem to prove that if there are specific and highly specialized end-organs each for warmth and for cold, they cannot be found by methods available at present. One is led to the suspicion that the same end-organs, under different field conditions, respond to cold and to warm stimuli, and that these organs are not far different from those of pain. At any rate the tradition that Ruffini's cylinders and Krause's end-bulbs are exclusively the organs of warmth and of cold, respectively, can no longer be maintained.

Bazett[3] points out that the temperature receptors may be located deeper under the surface of the skin than was supposed. This may explain why Dallenbach did not find organs under the sensitive spots of the skin, for he did not section deep underlying tissues. Should all the organs for warmth and cold be located *underneath* the layers investigated by Dallenbach, the available evidence in regard to their functioning would still point to the importance of field properties.

This study was undertaken to determine whether, under any condition or with any degree of refinement of technique, maps of cold and warm spots localized upon the surface of the skin were capable of repeated verification. As the study progressed and exact localization of the spots seemed possible, the opportunity was taken to try to identify the functional end-organs of cold and warmth, and thus to test histologically the validity of Von Frey's correlation.[4]

I. Localization and Mapping

The central problem of this study—localization and mapping of the temperature spots—grew out of our failure in the Cornell undergraduate laboratory to obtain a high degree of correlation between the results of successive mappings.

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[1] Parallels *The Science of Psychology*, Chapter XIV.

[2] By Karl M. Dallenbach, Cornell University. From the *American Journal of Psychology*, Vol. 39, 1927, 402, 404, 408, 409, 410-417, 418, 419-427. By permission. Certain tables and figures omitted.

[3] Bazett, H. C. "Physiological Responses to Heat," *Physiological Review*, 1927, Vol. 7, 531-599.

[4] M. von Frey, Beiträge zur Sinnesphysiologie der Haut, *Ber. d. kgl. sächs. Ges. d. Wiss., math.-phys. Kl.*, 47, 1895, 181-184.

The theoretical importance of the localization of the temperature spots, is very great. If the spots cannot be definitely localized, if successive surveys always yield different maps, if there is no consistency in the localization of the spots, then—assuming the technique to be faultless—our theories of cutaneous sensation would have to be revised, and our theories of nerve excitation and neural conduction would have to be considerably modified.

The technique, however, was very soon discovered not to be without faults. Indeed, this investigation might well be called a "study in errors," for it quickly resolved itself into a problem of obtaining an adequate technique for the work. One kind of apparatus after another, and one method after another, were tried and discarded. It was only after many failures that an apparatus and a procedure were devised that gave unequivocal results.

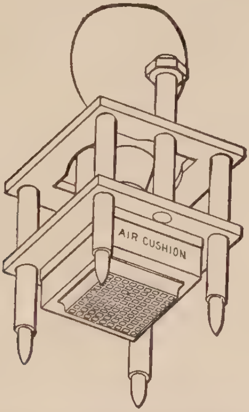

FIGURE 33 (Original FIG. 1). Boring's Precision Cross-section Stamp

SOURCES OF ERROR NOTED IN THE PRELIMINARY EXPERIMENTS

We noted in the preliminary experiments the following sources of error: (1) changes in the relation of the cutaneous and subcutaneous tissues; (2) volumetric changes of the tissues; (3) the instability of the grids stamped upon the skin; (4) the elasticity and stretch of the skin under the point of the temperature cylinder; (5) the difficulties of localization; (6) the pressure of the stimulus; (7) the duration of the stimulus; (8) radiation; (9) summation; (10) the positive after-image; (11) fatigue; and (12) "tuning."

(*1*) *Changes in the tissue relations.* The relation of the skin to the subcutaneous tissues changes with every movement. The more heterogeneous the tissues, as those of the back of the hand, the greater is the effect of movement; but even relatively homogeneous tissues such as those of the upper arm are affected by positional changes, for the adequacy of the temperature stimulus depends in part upon the nature of the subcutaneous background. If a given "spot" on the skin (to take an extreme case) is over a bone at one experiment, over a blood vessel at the next, over a tendon at the third, and over soft connective tissue at the fourth, the effect of the stimulus was found to differ greatly in the four cases. Neither the pressure nor the temperature differential would be constant upon these four backgrounds. As described above, we controlled this variable by using relatively stable tissues and plaster-cast arm-rests.

(*2*) *Volumetric changes.* Due to causes that we were unable to ascertain and to control, the volume of the tissues, particularly those of the upper arm, changed slightly from day to day. Whether these changes affected the "tuning" and functioning of the temperature end-organs we are unable to say, but they did complicate the problem of localizing the temperature spots. The grids, which were perfect squares when stamped upon the skin, were distorted in every direction. As long as we worked with the original grids—though distorted—and localized the temperature spots upon them, the volumetric changes did not greatly affect the results. Since a grid once stamped upon the arm could not—because of the volumetric changes—be replaced if smudged or effaced, our problem became one of maintaining the original imprints for the duration of the experiment.

(*3*) *Instability of the grids on the skin.* The abrasion of the clothes and the absorption of the skin rendered it difficult to retain the original grids upon the skin. Yet, because of the volumetric changes, the original grids must be retained. After many attempts we solved this problem, as described above, by using shields made of rings of felt.

(*4*) *Elasticity of the skin.* When the temperature-point is drawn across the surface of the skin by the "continuous" method, the skin, which is elastic and loosely connected to the subcutaneous tissues, tends to be displaced in the direction of the movement. This not only adds to the difficulty of localizing the "spot" discussed in (*5*) below, but it also augments the errors caused by the changes in the tissue relations discussed in (*1*) above. The errors due to the elasticity and stretching of the skin could be avoided if a discrete or "punctiform" method of stimulation were used in place of the "continuous" method.

(*5*) *Difficulties of localization.* The difficulty of localizing the exact "spot" at which the sensory experience was aroused is very great when the "continuous" method of stimulation is used. This is due in part to the displacement of the skin as mentioned in (*4*) above, but it is principally a matter of reaction-time. The stimulus-point, for example, passes over a sensory spot, the end-organ responds, an afferent impulse is set up, *O* experiences the sensation, gives his report, *E* apprehends it, and then notes the position of the stimulus-point and marks it as the "spot" at which the sensation was aroused. During the time interval of these reactions, approximately ½ sec. if we regarded them as two simple sensory reactions (which of course they were not, as neither *O* nor *E* were under reaction-instructions),[5] the stimulus-point was continuously moving. The "spot" that *E* marked would be displaced, therefore, from the "true" spot, and the amount of displacement would vary according to the rate at which the stimulus was moving, the reaction-time of *O*, and the reaction-time of *E*. We had no objective control of these moments, but it is certain that they were not constant, and that a variable as well as a constant error was involved in the localization and plotting of the "spots."

The temperature stimulator, moreover, was not adapted for fine discriminations in localization. Because the conical point of the water-bath chamber was blunt, the apical tip which served as the stimulus-point was not sharply delimited and *E*'s vision of it was partially obscured. *E* could not therefore determine exactly the position of the stimulus at the time of *O*'s report.

The probable error of *E*'s localization is a fourth source of error that must be taken into account when comparing the successive maps. Assuming that every other source of error was controlled, the probable error alone would suffice to introduce discrepancies. They would be small, to be sure, but they would be finite, consequently we should never get absolutely identical maps.

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[5] A. Goldscheider, Ueber die Reactionszeit der Temperaturempfindungen, *Arch. f.* [*Anat. u.*] *Physiol.*, 1887, 468-472.

The first two difficulties of localization are inherent in the "continuous" method of stimulation—both would be eliminated if the "punctiform" method were used. The size of *E*'s probable error in localizing the "spots" would, moreover, be smaller in the "punctiform" method, and it would be decreased still further if the point of the temperature stimulator were so constructed that *E* could more readily determine its exact position on the skin at the time of *O*'s report.

Another source of error in the "continuous" method is pointed out by Titchener. He says, "errors are apt to be made in localization, by the fact that *approach* to a cold or warm spot will set up a weakly cold or warm sensation. Even after repeated cautions, a student is likely to enter this approximation-sensation in his maps rather than the more intensive sensation which he can obtain by moving the cylinder a little farther, until it lies exactly over the temperature spot."[6] "Punctiform" stimulation would reduce the variability of such localizations, and training the *O*s would eliminate it.

(*6*) *Pressure.* It was utterly impossible, by the "hand and eye" method that we used, to carry the stimulator across the experimental area without varying the pressure of the stimulus-point upon the skin. Since the adequacy of the stimulus is in part determined by the pressure with which it is applied, it became incumbent upon us to devise some objective means of controlling this variable.

(*7*) *Duration.* The duration of the stimulus, in the "continuous" method, is a function of the rate and uniformity of movement. Though we tried to move the stimulus-point across the skin at a steady and uniform rate, it was obvious that we were unable to do so. Not only did the absolute time of the movement across the skin vary, but, because of the elasticity, looseness, and stretching of the skin mentioned in (*4*) above, the rate of movement within any one transit was not always uniform. Since the adequacy of a temperature stimulus is in part determined by the duration of the stimulus, the necessity of controlling this variable is quite evident.

(*8*) *Radiation.* A temperature spot may be stimulated by radiation. If this occurs and the "spot" is localized by the position of the stimulus-point, a large variable error is introduced into the work. The possibility of stimulation by radiation was present in all our preliminary experiments. Indeed we think this source of error is inherent in the "continuous" method of stimulation. But over and above this methodological error our apparatus was not designed with this source of error in mind. The apical angle of the conical temperature chamber, the tip of which was used as the stimulus-point, was large. This not only hindered accurate localization of the point as discussed in (*5*) above, but it increased the liability of stimulation by radiation. For exact work the possibility of stimulation by radiation should be eliminated.

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[6] Titchener, *Experimental Psychology* (*qualitative*), *Instructor's Manual*, 82 f.

(*9*) *Summation.* Unlike the action of separate and discrete stimuli, every one of which was effective in its own right, stimulation by the "continuous" method has a varying effect—depending on what has gone before. As the temperature-point is drawn across the skin, the temperature differential between the stimulus and the skin is affected by radiation, as discussed in (*8*) above; and the temperature experiences aroused are partially obscured by the positive after-images discussed in (*10*) below. Both these factors are summative in their effects. The more prolonged the stimulation, the greater the effects of radiation and the greater the change in the temperature differential; and the greater the number of temperature experiences aroused, the greater the number of positive after-images and the greater their obscuring effect.

Punctiform stimulation, and a proper regard for the duration and rate of stimulation would avoid the errors due to summation.

(*10*) *Positive after-images.* Positive after-images of both cold and warmth during the course of the experiments were reported by all the *O*s. To what extent, if any, they influenced the results we cannot say; none of our *O*s, as far as we know, confused an after-image with a sensation in their reports, though of course there was always that possibility with the "continuous" method of stimulation. More serious than this possibility, however, is the distraction of *O*'s attention with a resulting increase of the reaction-time of his subsequent reports—a source of error discussed in (*5*) above.

If the distracting effect of the after-images is to be avoided the "continuous" method of stimulation must be given up. The stimuli should be applied discretely; and they should not follow one another until the effects of the previous stimulations have run their course.

(*11*) *Fatigue.* The size of the area that we used in the preliminary work was 4 sq. cm. (2 × 2 cm.). The felt shields described above would not protect an area of this size—the central part of the stamped area was obliterated before the conclusion of the series of experiments. But over and above this practical consideration was the question of fatigue. Though seated in a comfortable chair and in a comfortable position, the *O*s became fatigued, restless, distracted, and ill at ease before the entire area had been worked over.

The errors of ''fatigue'' could be reduced by increasing the rate of stimulation, or by decreasing the extent of the experimental area. An increase in the rate of stimulation would augment the errors discussed in (*4*), (*5*), (*7*), (*9*), and (*10*) above, whereas a decrease in the size of the experimental area would leave these sources unaffected, and would bring no others in its train. The verification of the maps of small areas would be as convincing as that of large, accordingly the experimental area was reduced to 1 sq. cm.

(*12*) *''Tuning.''* The temperature spots are not all equally sensitive. They are ''tuned'' or adjusted so that they respond differently to a given stimulus. Unless, therefore, the ''tuning'' is constant the adequacy of the stimulus may vary even though the stimulus itself be constant. Not much is known of the phenomenon of ''tuning'' but it is probably dependent upon the temperature of the skin, *i.e.*, the physiological zero, and the general health of the *O*. We attempted to control both of these factors: the general health of the *O*s by having them, during the Main Experiments, follow a fixed routine; and the temperature of the skin by controlling the temperature of the experimental room. The observation hours were held regularly three times a week and at the same hour of the day, consequently the *O*s came to the different sittings with approximately the same degree of fatigue and temperature adaptation.

The reason that high degrees of correspondence between the results of successive mappings of the cold and warm spots have not heretofore been attained is evident. The apparatus, technique, and method of procedure were inadequate to the problem. The correspondences that have been obtained are, in view of the many sources of error that we have pointed out, more surprising than the failures.

MAIN EXPERIMENTS

We tried in the final series of experiments to make the mapping of the temperature spots a mere matter of stimulus and response. To this end we attempted to eliminate all the errors noted in the preliminary work: we reduced the area explored; we devised a new thermal stimulator; returned to the punctiform method of stimulation used by Blix in his pioneering work,[7] and modified the procedure.

Apparatus. In the final series of experiments a new thermoæsthesiometer was used that was designed with the particular needs of our problem in mind. This apparatus is shown in outline in Fig. 34. As it has been fully described elsewhere,[8] a statement of the salient features will here suffice.

The stimulus-point *A*, a copper cylinder 1 mm. in diam., projects 3 mm. from the point of the cone-shaped temperature chamber *B*. The extension of the point makes for more accurate localization—the point is sharply demarcated and is not obscured by the housing of the temperature chamber; and it also reduces, if it does not actually eliminate, stimulation by radiation from the temperature chamber. The temperature of the stimulus-point is controlled by a current of water carried to chamber *B* by tube *C*, and away by tubes D_1 and D_2. These conducting tubes form the framework for the handle of the apparatus. The handle *F*, which is carried by the central tube *C*, is tapered at the lower end so its position as it is moved up and down tube *C* may readily be observed on scale *H*. Spring *G*, which stands between the handle and the connecting bar *E*, carries the weight of the apparatus when it is held in a position ready for stimulation. The spring and the sliding handle make it possible to control the pressure of the stimulus. When the stimulator is applied the weight of the apparatus is gradually transferred from the spring to the skin. The proportion of the weight transferred is a function of the release of the spring, *i.e.*, the distance the handle is lowered on scale *H*. If the handle is always lowered to a given point on scale *H* the pressure of the stimulus on the skin will be constant.

The water supply for the apparatus was obtained directly from the University mains and was forced through the stimulator and the connecting rubber tubing by the pressure of the mains. In the experiments with warmth, the water was first led through a copper coil in a constant-temperature bath, but in the experiments with cold the water was led directly to the

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[7] Magnus Blix, Experimentelle Beiträge zur Lösung der Frage über die specifische Energie der Hautnerven, *Zsch. f. Biol.*, 20, 1884, 141-156.

[8] K. M. Dallenbach, Some new apparatus, *Amer. J. Psychol.*, 34, 1923, 92-94.

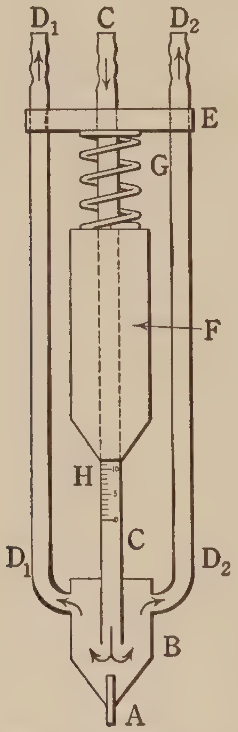

FIGURE 34 (Original FIG. 2)
Thermo-æsthesiometer

stimulator. As in the preliminary experiments the temperature of the water was taken both before and after it had passed through the stimulator, and the average of the two readings was assumed to be the temperature of the stimulus-point. This may not have been correct, but it sufficed for our needs. We were interested not in the precise temperatures, but in their constancy and adequacy. By properly regulating the temperature of the room and the flow of the water through the apparatus, the temperature of the stimulus-point remained practically constant for any given hour—the variation for a given hour was never more than 1/5° C. The temperature of the stimulus-point at the different experimental hours varied, as in the preliminary experiments, between greater limits; the cold was at 8–9° C, and the warm was at 42–44° C. The temperature of the room on the different days varied between 19.8–22.6° C.

Observers. With the exception of B, whose skin as we have remarked proved peculiarly susceptible to the irritation of the felt shields, the *O*s who served in the preliminary experiments continued in the final series. They were all now highly practiced in the observation of the cold and warm experiences. They observed at five periods on the main experiments—on Monday, Wednesday, and Friday of one week and Monday and Wednesday of the next—and, as in the preliminary experiments, on the same hour of the day at every sitting. Since the *O*s were all following a routine schedule, the regularity of their observation was a guarantee that they brought to the experiments a cutaneous surface that had been uniformly "tuned," that had in other words been subjected to approximately the same amount of adaptation and fatigue.

Procedure. After *O* had been seated in a comfortable chair with his left arm, hand, and fingers resting on a table—in a plaster-cast that had been made specially for him—a grid, 1 cm. sq., with cross-section lines 2 mm. apart, was stamped upon his upper arm. We chose an area that had relatively few and unevenly distributed hairs. After the location of the hairs was mapped, the area was shaved, and the felt-ring shields were fastened with celloidin to the skin around the grids. Nothing further was done on this day. The chair and table were fastened to the floor, and the position of *O* in the chair and of his plaster-cast on the table were marked. *O* could therefore return to approximately the same position at the different experimental hours.

The "punctiform" method of stimulation was used. We began with the 2 mm. square in the upper left-hand corner of the experimental area and worked across the first row of squares, then we worked across the second row of squares, then the third row, and so on. Stimulation within every square followed in the order shown in Fig. 35. The stimulus-point was placed in the upper left-hand corner of the first square, then in the upper right-hand corner, then in the lower right-hand corner, and lastly in the lower left-hand corner. After which the second square was explored in like manner, and so on until the entire area had been worked over.

The pressure and duration of the stimulus was constant throughout the course of the experiment. Uniformity of pressure was secured by means of the spring control on the stimulator described above; constancy of duration was secured by means of a silent second's pendulum that *E* kept track of by peripheral vision and subvocal counting. The stimulus was applied at the count of one, removed for the counts of two and three, applied again at one, and so on, until the first row of squares had been stimulated, at which time a short rest was taken so *O* could break his sustained attention. A "ready," "now," signal was given before the first stimulation of every row of squares, but thereafter none was given, the stimuli themselves served as their own preparatory signal.

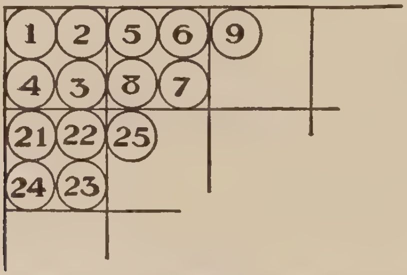

FIGURE 35 (Original FIG. 3). Procedure in Exploring Successive Squares of the Grid

The stimulator was applied with *E*'s right hand, with his wrist resting upon *O*'s plaster-cast arm-rest. The stimulus-point could therefore be applied steadily and accurately. When *O* reported the arousal of a "cold" or a "warm," the position of the "spot" was marked by *E*, with his left hand, on an enlarged map, without interrupting the course or rate of the stimulus-series.

Four maps were made of both the cold and the warm spots. We had planned to make five, but the condition of the grids and the skin after the fourth sitting did not warrant a further postponement of the excision of the temperature spots—an extension of the experiment that we decided upon when it became evident that the exact localization of the spots was possible. The cold spots were mapped first at every hour, and then after a short rest during which *O* was free to move around the room as he pleased, the maps of the warm spots were made.

Results. The maps that were obtained at the different sittings are reproduced in Figures 36 and 37, for D and R, respectively. The number of cold and warm spots obtained at every mapping is given in Table II.

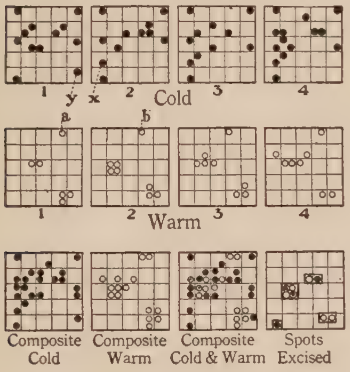
1
y
x
2
Cold
3
4
a
b
1
2
Warm
3
4
Composite Cold
Composite Warm
Composite Cold & Warm
Spots Excised

Figure 36 (from original Plate I). Maps of Cold and Warm Spots for D. Area 1 sq. cm.

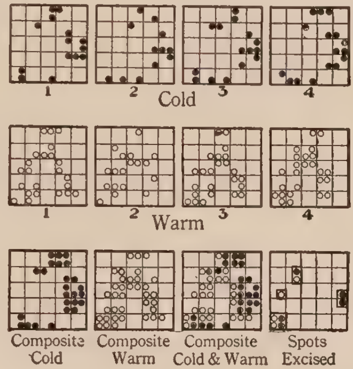
1
2
Cold
3
4
1
2
Warm
3
4
Composite Cold
Composite Warm
Composite Cold & Warm
Spots Excised

Figure 37 (from original Plate III). Maps of Cold and Warm Spots for R. Area 1 sq. cm.

Table II

Number of Cold and Warm Spots Aroused at the Different Sittings
Area 1 sq. cm.

	Maps of Cold Spots					Maps of Warm Spots				
O	*1*	*2*	*3*	*4*	*Av.*	*1*	*2*	*3*	*4*	*Av.*
D	12	11	11	14	12.00	6	8	8	8	7.50
G	19	21	16	15	17.75	20	20	19	20	19.75
R	14	12	15	16	14.25	23	18	24	25	22.50

D reported, for the particular location that we selected, an average of 12 cold spots and 7.5 warm spots; G reported an average of 17.75 cold spots and 19.75 warm spots; and R reported an average of 14.25 cold spots and 22.50 warm spots. D shows a slight difference in favor of the cold spots, G a slight difference in favor of the warm, and R a marked difference in favor of the warm. These results stand in direct opposition to statements appearing in current textbooks that the cold spots greatly exceed the warm in number,[9] and to the results that have been obtained by the "continuous" method in other experiments.[10]

The discrepancy between our results and those previously obtained may be due to individual differences, to areal differences, or to differences in technique. Though there is large variation among the results of our *O*s, all of them report warm spots greatly in excess of the number expected. We hardly believe, therefore, that individual differences will explain the discrepancy. Neither do we believe that areal differences will explain it, for the upper arm is not peculiarly sensitive to warmth, and we should expect the same relation to hold there as upon other surfaces of the skin. The method of stimulation seems therefore to be the sole basis of the results.

Whether our results are artifacts of the "punctiform" method, or the results of other investigators are artifacts of the "continuous" method, we are not prepared to say. It is certain, however, that the distribution of the cold and warm spots, as shown in Figures 36 and 37, is such that had the "continuous" method been used the usual results would have been obtained. The cold spots are more or less separate and discrete, stimulation by the "continuous" method would therefore have separately aroused them. The warm spots, on the other hand, seem to be arranged in groups. D, for example, gives three distinct clusters, and G and R each five. If these clusters are stimulated by the "continuous" method there would be a strong tendency to consider the group as a single, large diffuse "spot." Which is the true picture, *i.e.*, whether there are a few large warm spots or numerous small warm spots grouped into clusters, we are unable to say, for the results seem so obviously a function of the method. The answer, in our opinion, awaits the identification of the physiological correlate.

The per cent. of correspondence between the different maps of the cold and warm spots, is given in Tables III and IV.

The values given in Table III were obtained by the "student method" by which the correspondence of one map to another is determined by the percentage of spots that are identically localized or that fall within 1 mm. of one another. For example, D reported 12 cold spots at the first mapping (Figure 36,

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[9] Titchener (*A Text-book of Psychology*, 1909, 150) gives the proportion of cold to warm spots as 13 to 3; Ladd and Woodworth (*Elements of Physiological Psychology*, 1911, 346) place it at 10 to 1.

[10] G. Sommer, Ueber die Zahl der Temperaturpunkte der ausseren Haut, *Sitzber. d. phys. med. Gesell. Würzburg*, 1901.

Cold 1), and 11 at the second (Figure 37, Cold 2); 11 of the 12 spots in the first map "corresponded," in this meaning of the term, to spots in the second map, and 10 of the 11 spots in the second map corresponded to spots in the first. The correspondence of D's first map to his second is therefore 92%, and of his second to the first 91%. The correspondence of the two maps is the average of these, 91.5%.

TABLE III

PER CENT. OF CORRESPONDENCE BETWEEN THE VARIOUS MAPS OF THE COLD AND WARM SPOTS

Computed by the "student method"

	Maps of	*Correspondence of*													
O	*Spots*	*1-2*	*2-1*	*1-3*	*3-1*	*1-4*	*4-1*	*2-3*	*3-2*	*2-4*	*4-2*	*3-4*	*4-3*	*Av.*	*m.v.*
D	Cold	92	91	92	91	92	86	100	100	91	93	100	100	94.0	± 4.0
	Warm	100	100	100	100	100	88	100	100	100	88	100	100	98.0	± 3.3
G	Cold	100	95	100	100	95	100	90	100	86	100	94	100	96.6	± 3.9
	Warm	100	100	100	100	100	100	100	100	100	100	100	100	100.0	± 0.0
R	Cold	100	92	100	93	100	94	100	100	100	100	100	100	98.2	± 2.6
	Warm	96	100	100	100	100	100	94	100	94	100	100	100	98.7	± 2.0

TABLE IV

PER CENT. OF CORRESPONDENCE BETWEEN EVERY TWO, AND OF ALL THE MAPS OF THE COLD AND WARM SPOTS

Computed by the "absolute method"

	Maps of the Cold Spots							*Maps of the Warm Spots*						
O	*1-2*	*1-3*	*1-4*	*2-3*	*2-4*	*3-4*	*All*	*1-2*	*1-3*	*1-4*	*2-3*	*2-4*	*3-4*	*All*
D	93	91	91	90	89	91	82	93	94	96	94	92	94	88
G	84	87	90	87	88	93	78	88	85	80	97	88	89	81
R	84	87	86	93	90	91	76	84	89	82	84	81	89	73

The correspondences for every two of the four cold maps range for D from 86 to 100%, averaging 94%, they range for G from 86 to 100%, averaging 96.6%, and they range for R from 92 to 100%, averaging 98.2%. The correspondences for every two of the four warm maps range for D from 88 to 100%, averaging 98%, they range for R from 92 to 100%, averaging 98.7%, and for G the correspondences are uniformly 100%.

It may be, in view of the many sources of error in localizing and mapping the temperature spots, that the "student method" of computing correspondence is as precise as conditions warrant; but the method seems to us to be defective in two ways. In the first place, all the spots on the two maps between which a correspondence is being figured are not taken into account in the separate computations. Take, for example, the case of the correspondence of D's cold maps 1 and 2, mentioned above. The correspondence of map 1 to map 2 was first computed, then the correspondence of map 2 to map 1 was computed, and then the two values thus obtained were averaged, and the average was regarded as the correspondence between the two maps. But in figuring the correspondence of map 1 to map 2, the spots on map 2 were taken into account only in so far as they corresponded to spots on map 1. There is one spot in map 2 (marked *x* in Figure 36, Cold 2) that has no correspondence in map 1, and it was disregarded in the computation of the correspondence of map 1 to 2. Similarly when the correspondence of map 2 to map 1 was computed. There is one spot in map 1 (marked *y* in Figure 36, Cold 1) that has no correspondence in map 2, and it is disregarded in the computation of the correspondence of map 2 to map 1. The averaging of the correspondences thus obtained does not eliminate these separate errors; it compounds them. In the second place, the "blanks," the spots at which no temperature experiences were reported, are disregarded in the "student method." The fact that neither warm nor cold was reported at a given spot is just as significant, we feel, as a positive report. A "blank" is a report, and the consistency with which "blanks" appear should be taken into account in computing the correspondence of two maps.

The correspondences given in Table IV, of the cold and the warm maps, were computed by what we shall call the "absolute method." In this method, since there were 100 sq. mm. in every map, we simply counted the number of identical squares in the maps whose correspondence we sought. For example, there were 93 identical millimeter squares in D's first and second cold maps, 91 in his first and third maps, 91 in his first and fourth maps, 90 in his second and third, 89 in his second and fourth, 91 in his third and fourth, and 82 of the squares were identical in all four of his maps.

Correspondence between the different pairs of cold maps ranges from 89 to 93% for D, from 84 to 93% for G, and from 84 to 93% for R. The correspondence among all four of the cold maps is 82% for D, 78% for G, and 76% for R. Correspondence between the different pairs of warm maps ranges from 92 to 96% for D, from 85 to 97% for G, and from 82 to 89% for R. The correspondence among all four of the warm maps is 88% for D, 81% for G, and 73% for R.

The "absolute method" of computing correspondences between maps is, insofar as it is an "all or none" method, inadequate to the problem. Unless the spots are identically localized upon the different maps they are not regarded as corresponding. Spots adjacently localized are not only counted as complete misses, but they reduce the correspondence more than if "blanks" had been reported. To be specific, consider spots *a* and *b* in D's Warm Maps 1 and 2 (Figure 36). These spots are adjacent. The appearance of the spot at *b* on map 2 is significant, much more so than if a "blank" had been reported there, yet the spot at *b* reduces the correspondence, whereas a "blank" would have increased it. This is an obvious defect that can be corrected only by adopting a system of partial credits or weights for the computation of the correspondence of adjacent spots. Since any system of weights proposed would be arbitrary, we leave the question of method with the observation that the "absolute method" gives correspondences that are lower than the data warrant, and the "student method" correspondences that are based upon part of the data only.

The correspondences that we have obtained between the various pairs of the maps of the cold and warm spots, and among all four of the maps of the cold and warm spots, is so high for all the *O*s that we feel justified in claiming that we have demonstrated the fact that it is possible, under standardized conditions and with an adequate technique, to obtain repeatedly verifiable maps of the cold and warm spots.

Corollary results. (*1*) *Warm spots definite.* The correspondences that we obtained were higher for the warm spots than for the cold. The average correspondences computed by the "student method," were: D, warm maps 98%, cold maps 94%; G, warm 100%, cold 96.6%; R, warm 98.6%, cold 98.2%. The correspondences among all four of the cold and all four of the warm maps, computed by the "absolute method," were: D, warm maps 88%, cold maps 82%; G, warm 81%, cold 78%; R, warm 73%, cold 76%. These results are contrary to what we had expected,[11] but the *O*s were consistent and their introspective reports agreed with these objective findings. During the early weeks of the experiments the *O*s gave the customary descriptions of the cold and warm qualities,[12] but as the experiments progressed and they became more proficient in the work and more familiar with the temperature qualities, the indefiniteness of the warm experiences disappeared. The *O*s found, as the objective results show, that the warm spots were more easily and more definitely identified than the cold. It is only an observer who is naïve in the introspection of the qualities of the temperature spots, who describes his warm experiences as “indefinite,” “vague,” “illy defined,” “less insistent,” “more difficult to localize than the cold.”

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[11] Von Frey says, "Alle Beobachter stimmen darin überein, dass die genaue Lagebestimmung für die Warmpunkte viel schwerer ist, als für die Kaltpunkte" (*op. cit.*, 183). The reports of the beginning students in the Cornell qualitative laboratory, and of our *O*s during the early part of the experiment reported here, confirm this finding. The greater definiteness of the warm spots was, therefore, entirely unexpected.

(2) *Map of the hairs.* A comparison of the maps of the hairs with the maps of the cold and warm spots failed to reveal any relationship between the hairs and the temperature spots. The hairs appeared to be scattered haphazardly among the cold, the warm, and the “blank” spots on the maps of all the *O*s. A composite map of the hairs, of the cold, and the warm spots, is shown for D in Figure 38. The composite maps of the other two *O*s are omitted for lack of space. We could find no evidence in our results for Goldscheider's contention that the temperature spots radiate from centers coincident with the papillæ of the hair.[13]

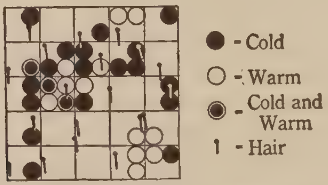

FIGURE 38 (from Original FIG. 4). D's Composite Map of the Hairs, Cold Spots, and Warm Spots

II. THE HISTOLOGICAL EXAMINATION OF THE COLD AND WARM SPOTS

When it became evident that precise and accurate localizations of the warm and cold spots were possible, we decided to test the correlation drawn by Von Frey between the Krause end-bulbs and cold, and the Ruffini cylinders and warmth.[14] Certain of the spots, therefore, that had given uniform responses in all of the mappings, were excised, fixed, sectioned, stained, and examined microscopically.

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[12] Titchener, *A Text-book of Psychology*, 1909, 150; *Experimental Psychology, Qualitative, Instructor's Manual*, 1901, 82.

[13] Goldscheider, *op. cit., Arch. f.* [*Anat. u.*] *Physiol.*, 1885, 340-345.

Four areas were excised from every *O*. The particular areas selected are shown in Figures 36 and 37, in the maps above the legend "Spots Excised." One of the areas contained a cold spot isolated from the warm spots, a second contained a warm spot isolated from the cold spots, and the other two areas excised contained both warm and cold spots. A composite cold map, a composite warm map, and a composite cold and warm map are shown in Figures 36 and 37, for D and R, respectively. The background and the spot relationships of the excised spots can therefore clearly be seen.

The spots were excised by a surgeon.[15] The operation was performed with *O* in his usual position at the experimental table with his left arm in the plaster-cast arm-rest. The tissue relations were therefore the same during the operation as during the mappings of the spots. Four incisions, vertical to the surface of the skin, were made on the four sides of the rectangular areas that were to be removed. The incisions extended deep (approximately 3 mm.) into the fascia; we wished to get all the tissue that might possibly contain the end-organ. The block of tissue was removed by cutting it off square at the bottom through the fascia.

The excisions were made without an anæsthetic. We could not use a general anæsthetic and have the *O*s seated during the operation in their customary positions at the experimental table—a condition which our experiences in the preliminary work seemed to indicate as very necessary. We hesitated to use a local anæsthetic because we did not know what effect it would have upon the tissue and end-organs. We did not wish at the last moment to introduce anything that might invalidate the results; so we let the *O*s suffer—which they manfully and willingly did!

The blocks of excised tissue were placed in Zenker's fixing fluid immediately upon their removal. After washing, and running them rapidly through the alcohols, they were dehydrated, imbedded in paraffin, serially sectioned (the sections were 10μ thick), and stained with methylene blue.[16]

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[14] M. von Frey, Beiträge zur Sinnesphysiologie der Haut, *Ber. d. König. sächs. Gesell. d. Wiss., math.-phys. Kl.*, 47, 1895, 181-184.

[15] My thanks are due Dr. Otto E. Utzinger for his co-operation and assistance.

Results. The microscopical examination of the serial sections revealed numerous nerves and undifferentiated nerve-endings, but not a single specialized end-organ was discovered.

Negative results, such as ours, are of course inconclusive. The logic of Von Frey's correlation is not affected by them; but as long as the experimental results are negative, and there is nothing more tangible than the logical evidence advanced by Von Frey, we feel that the psychologists and physiologists are not justified in teaching as established fact that Krause's end-bulbs function for cold, and Ruffini's cylinders for warmth.

Von Frey did not perform the "crucial" experiment.[17] He had, therefore, no direct experimental evidence for his correlations. His theory is entirely a logical matter. It is based upon the fact that the end-organs which he correlates with the different sensory qualities are found in places at which those qualities are especially prominent or exclusively present. He concludes, for example, that Krause's end-bulbs, which are found in the conjunctiva, the margin of the cornea, different parts of the mouth-cavity, the glans penis and clitoris, are temperature end-organs because the conjunctiva and the glans lack pressure sensation, and that they are cold end-organs because the margin of the cornea lacks warm sensations; that Ruffini's cylinders, which are found in the finger, upper arm, and eyelid, are warm end-organs because the eye-lid is particularly susceptible to warmth.

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[16] This part of the work was done in the Histological Laboratory of Cornell University. My thanks are due Professor B. F. Kingsbury for his advice and assistance, and for the facilities of his laboratory that he placed at my disposal.

In the preliminary study of the Krause end-bulbs of a cat's conjunctiva, we used several fixing fluids—Zenker's, Hermann's, Flemming's, and 10 per cent. formalin; and several kinds of stain—methylene blue, gold chloride, hematoxylin, and picro-fuchsin. We did not observe much difference between the effects of the fixing fluids, but we were able to differentiate the Krause end-bulbs of the cat's conjunctiva more clearly with methylene blue than with any of the other stains.

[17] The "crucial" experiment, *i.e.*, the histological examination of the tissue under a cold or warm spot, was first performed by Donaldson in 1885. His results were negative, he failed to find any specialized end-organs. "No difference," he says, "could be made out between the spot at which cold had been felt and that at which heat was observed. There were numerous nerves beneath these spots but these were almost as numerous in neighboring parts. The results of the histological investigation then is so far completely negative" (H. H. Donaldson, On the temperature sense, *Mind*, 10, 1885, 408 f.).

Howell says in 1905, "Many efforts have been made to determine whether there is a specific kind of end-organ for each of these senses [cold and warmth]. Numerous observers have cut out the skin from cold or hot spots and examined the removed part carefully by histological methods. The general result has been that no distinctive end-organs have been found" (W. H. Howell, *A Textbook of Physiology*, 1905, 260). These statements are repeated, without references, in every edition of Howell's book. Though I have searched long, I have been unable to find the reports of these experiments. It may be that Howell is referring to unpublished results that were privately communicated to him. But be that as it may, the second report of the "crucial" experiment that I have been able to find was given by Häggqvist in 1913. Häggqvist's results were also negative.

Von Frey was duly cautious and conservative in stating his theory. He said, "the end-bulbs are probably for that reason the organs of cold sensation";[18] "there appears to me a probable correlation between Ruffini's end-organs and the sense of warmth."[19] He was, moreover, straightforward in pointing out the defects and weaknesses of his theory. He says, regarding the correlation between cold and Krause's end-bulbs, "A serious difficulty apparently arises from the acceptance of the view, because the capacity to feel cold sensation belongs not only to the places enumerated but also to the outer skin."[20] And again, "Whether the number of end-bulbs in the skin suffices to accommodate the verifiable cold spots is a question which still remains to be decided."[21]

The further we get away from Von Frey the more dogmatic in general become the statements regarding his correlation. The writers of textbooks during the first decade following the formulation of the theory were reserved in their statements concerning it, in the next decade they were less so, and during the past decade the writers have very frequently given the correlation as established fact.[22]

Since we found no specialized end-organs, but only undifferentiated nerve-endings in the serial sections, our results would seem to indicate that the free nerve-endings besides functioning in pain also subserve the functions that Von Frey ascribed to Krause's end-bulbs and Ruffini's cylinders.

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[18] *Op. cit.*, 182.

[19] *Ibid.*, 184.

[20] *Ibid.*, 182.

[21] *Ibid.*

[22] Lack of space forbids that I list all who sin in this respect, and fairness forbids that I single out any group. Instances, however, are so numerous that citations are unnecessary!

GLOSSARY

ABRASION, a rubbing off (or through).
APICAL ANGLE, angle at the apex of.
CORNEA, transparent wall of the eye-ball in front of the pupil; the external refracting medium of the eye.
DEHYDRATE, to remove water from.
E, experimenter.
FIXING FLUID, fluid in which tissue is soaked in order to make it firm enough to be cut into microscopically thin layers; hardening fluid.
GLANS CLITORIS, erectile part of female reproductive organ.
GLANS PENIS, end of male reproductive organ.
METHYLENE BLUE, a kind of blue dye with which certain tissues can be stained to make the details of structure stand out.
μ, a thousandth of a millimeter; millionth of a meter; micron.
O, observer.
PHYSIOLOGICAL ZERO, temperature of the temperature organs at which no sensory processes of warmth or cold are felt; temperature to which the receptors for warmth and cold are adapted and do not respond.
SUBCUTANEOUS TISSUE, tissue lying directly beneath the skin.

GROUP VII

THE NERVOUS SYSTEM

READING TWENTY-SIX[1]

STUDIES OF CEREBRAL FUNCTION IN LEARNING VI

THE THEORY THAT SYNAPTIC RESISTANCE IS REDUCED BY THE PASSAGE OF THE NERVE IMPULSE[2]

Editor's Note

The Problem of Localization of Function.—As a consequence of numerous investigations, most of them by Professor Lashley, configurational theories of brain physiology are replacing the older emphasis upon a strict *localization of function* in different parts of the brain. When one can stimulate a certain area of the cortex and elicit a particular movement in the animal, or one of a limited group of movements, it is a strong temptation to conclude that the stimulated region is the "seat" or exclusive area for the control of those movements. When one can destroy another part of the cortex and always render the animal blind, temporarily, the evidence would seem definite enough that the region in question was *the* natural area *for vision.* But in coming to such conclusions as these, several points are overlooked. Cut a nerve in the arm and a part of the arm is made insensitive. Why not locate the seat of the lost cutaneous sensitivity in the *cut?* Destroy one part of a unified whole, one "link in the chain" and that whole is destroyed, but obviously just because this is true the functioning of the whole cannot be localized in any *one* of its parts.

If the brain always functions as a whole, one may ask, why does not a destruction of some *other* part than the visual area destroy the animal's vision? Take out its so-called auditory area and vision, seemingly, is not affected. The answer to this question is that the animal's vision depends upon a mass action of the cortex, and that a destruction of so small an area as the auditory does not *appreciably* destroy the mass action. As a matter of fact, it is not known whether vision is affected.

Professor Lashley has shown that destruction of *enough* of the cortex, in various places, reduces the animal's observed efficiency in *any* mode of behavior, but that a particular limited area is not essential, under all conditions, for any one type of performance. These results indicate that the brain as a whole is a balanced field of potentials and that the laws governing its activity are laws of the whole. When a certain mode of behavior is eliminated temporarily by an operation on the cortex, it simply means that a *field* formerly balanced as a whole, in a way to permit that behavior, is now disequilibrated. A new balance must be affected before the behavior will return, and this is probably what happens because the behavior does return. The rebalancing process takes time. It cannot be said, then, that the "seat" of the lost function has migrated to another *particular* part of the brain, or that another part has taken over the lost function, vicariously. The lost function, formerly dependent upon the brain as a whole, *returns when the remaining whole assumes the necessary balance.*

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[1] Parallels *The Science of Psychology*, Chapter XV or XVI.

[2] By K. S. Lashley, University of Chicago. From the *Psychological Review*, Vol. 31, 1924, 369-375. By permission. No. V of this series appeared in *Arch. Neurol. and Psychiat.*, 1924.

Considering the sense organs, routes from them to the brain, the brain itself, and routes from the brain to the muscles, all as a unitary dynamic field, the hypothesis of field properties makes conceivable many phenomena that the localization of function theory could not explain, for example, certain phenomena of hypnosis and hysteria. Under the right conditions a person may be conscious and yet not be able to see or hear, although there is nothing organically wrong with his eyes or ears. Perhaps he will be unresponsive to temperature or pain stimulation, yet nothing is organically wrong with the sense organs of the skin. Facts of this nature make untenable any theory which supposes that sense organs and fixed, limited parts of the brain constitute the machinery for the experiencing of sensations. One would have to assume that some mysterious barrier was raised all about the edges of these fixed areas, blocking them off, sharply, from the rest of the brain. A glance at the structure of the cortex would at once rule out this possibility, even if it were conceivable from a dynamic standpoint. The area is not an anatomical thing; it has no set wall of thresholds in the form of synapses, encircling it. Its synaptic connections bring it into anatomical relationships with all parts of the cortex. The only way in which a barrier of this sort could be set up would be to incapacitate all of the rest of the brain!

Again, therefore, one is thrown back upon the assumption of potentials that engage the brain as a whole. It may be supposed that, in hypnosis, the brain as a whole is disequilibrated. When a person by this means is rendered insensitive to pain it is conceivable that structures ordinarily involved in feeling pain are being used in keeping him from feeling pain, owing to the altered dynamic relationships brought about in the course of the hypnosis! In some way the insensitivity is conditioned by bringing the pain receptors into equilibrium with the brain as a whole, a relationship which cannot be expressed in terms of localization of function.

Thus it can be seen that Professor Lashley's work makes necessary a revision of many former theories of brain activity. Among them is the problem of the relationship between learning and the synapse. The apparent repudiation of the synaptic resistance theory of habit formation points again to the configurational character of brain-functioning. Whether or not a synapse will function depends upon its dynamic relationship to the surrounding areas, not upon the number of times nerve currents have passed through it.

Among the many unsubstantiated beliefs concerning the physiology of the learning process none is more widely prevalent than the doctrine that the passage of a neural impulse through the synapse somehow reduces synaptic resistance and leads to the fixation of a new habit. All theories which are based upon frequency of repetition imply that in learning there is a preliminary penetration of resistant neural paths by irradiation of propagated disturbances and a subsequent lessening of the resistance by repeated use of the new paths. Various elaborate theories concerning the mechanism of this change in resistance have been advanced, ranging from the chemical absurdities of Robertson (1) to the recent revival of the theory of dendritic growth of Kappers (2), but no direct evidence for changes in synaptic resistance has ever been obtained. The hypothesis is not based upon neurological data but is merely a restatement of the observed fact that increased efficiency follows repeated performance.

On the assumption that spinal reflexes represent the basic mechanism of all neural activity the hypothesis has some justification, being simple and in accord with the more apparent facts. It has led to important experimental studies and has furnished a simple pedagogical formula. But the evidence is far from convincing that the acquisition of a simple function is a gradual process or one dependent upon the passage of nerve impulses. All experiments must deal with complex functions involving the successive acquisition of many elementary connections. Familiar learning curves are obviously an expression of these successive integrations and we have no knowledge of the conditions prevailing in formation of a new simple neural integration. The instantaneous character of simpler associations in man, the "all or nothing" law which governs the appearance of some conditioned reflexes,[3] and the like suggest that in these cases a single performance serves to fix the habit. If this were the case for every simple reintegration within the nervous system we should still get the appearance of gradual improvement through practice, because of the successive formation of many such simple associations or the successive blocking of interfering integrations such as seems to take place in the perfecting of the conditioned reflex.

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[3] It is characteristic that the conditioned reflex is not built up gradually, but is fully integrated when it first appears. Improvement can be expressed only in terms of the proportion of times when the associated stimulus succeeds or fails to elicit it. It is at first easily inhibited. With practice the inhibition becomes more difficult, but there is some evidence for the belief that this is due to the building up of specific resistances to inhibiting agents rather than to a simple increase in the excitability of the associated reaction.

The fact of gradual improvement in complex functions cannot therefore be taken as evidence for a *gradual* wearing down of synaptic resistance by repeated passages of neural impulses and the hypothesis must still be considered as open to question. In recent experiments upon other phases of the neurology of learning, I have made incidental observations of some phenomena which seem altogether incompatible with this theory of the wearing down of synaptic resistance and which indicate that new conditioned reflex paths may be established under conditions where the passage of significant neural impulses over the arcs in question is precluded during the whole course of learning. The experiments deal with both centripetal and centrifugal paths, in both of which it has been possible to block the peripheral neurons during training and to observe their later functioning in the performance of the habitual acts.

Conditioning of Afferent Paths Which Were Nonfunctional During Practice

In tests of the mutual relationships of the striate areas of the cerebral cortex of the albino rat I effectually blindfolded the left eye of an animal with a velvet-lined wire and adhesive tape mask. The blindfold remained in place for a month during which the animal was trained in a Yerkes discrimination box to avoid the brighter of two lights. When discrimination was perfect (no error in 30 trials) the blindfold was transferred to the right eye. When the animal became adapted to the new adjustment his discrimination with the left eye was tested. He showed perfect discrimination with this eye which had not been used during the formation of the habit.

This result is no more than would be expected when we recall the facility with which we ourselves recognize an object with either eye when it has previously been seen with one eye alone (the common experience of the microscopist who views his field alternately with either eye). Yet this obvious fact has never been brought into relation to learning theory. Neural fibers come from the two retinæ and somewhere impinge upon a final common path. In the above experiment fibers from the right eye are integrated with the effector mechanisms of the avoiding reaction during the course of training, in which they have been subjected to retinal excitation. But the neurons of the left optic tract prove to be equally well integrated with the avoiding reaction, although that eye was not subject to photic stimulation during training.

It is not sufficient to say that this result is explained by the fact that fibers from corresponding points of the retinæ reach the same central ganglion cells. The corresponding fibers do not anastomose and there is no evidence that they have synapses in common. The wearing down of resistance in one afferent tract could not therefore affect the other. It is possible that the corresponding points activate the same central ganglion cells and that the reintegrations take place between these cells and lower motor neurons. But the following facts make this supposition rather questionable. Destruction of one eye and the contralateral cerebral visual area in the rat does not in the least affect visual discrimination. Destruction of one eye and the homolateral visual area reduces accuracy of discrimination. If the cortex contained ganglia equally integrated with both retinæ these operations should produce like effects by destroying the final common path. Since they do not, this hypothetical final common path must lie at a subcortical centrifugal level. (The data on hemianopsia in man also indicate that the final common path, except for pupillary reflexes, is not established at a precortical level.) The impulses to habitual discrimination must therefore pass through the cortex, since the destruction of both striate areas abolishes visual habits, before they reach such common ganglia and hence the destruction of the striate areas should abolish the functional identity of impulses from the two eyes.

I have repeated the above blindfold experiment with an animal after destruction of the visual area of both hemispheres (occipital third of the cortex). The blindfold was applied to the left eye immediately after the brain operation, so that there was no possibility of the formation of a new common association through the simultaneous use of the two eyes. The results of the test with this animal were the same as with the normal one; effective performance of the habit with the eye which had been blindfolded during training was obtained.

These facts seem to exclude the theory of a simple connection of corresponding points in the two retinæ with common ganglion cells and reintegration through the passage of impulses over this final common path. As an alternative, the drainage theory might be appealed to here; the hypothesis that the excitation of a neuron exerts a suction upon all the synapses of that neuron (3), but recent work on the character of the propagated disturbance in the axon has most thoroughly exploded the doctrine of a nervous energy or neurin. Moreover, the same phenomenon seems evident in recognition of tactile stimuli and no one would claim that all cutaneous afferent fibers have synapses in common or that the excitation of one final common path drains energy from all afferent tactile fibers.

It seems, then, that the theory of wearing down of synaptic resistances is inapplicable to this case of learning and that we must seek for some other principle to account for the altered conductivity of the neuron system.

THE UTILIZATION OF EFFERENT PATHS WHICH WERE NONFUNCTIONAL DURING LEARNING

More striking data with the same implications have come from studies of the precentral gyrus in monkeys. These have been reported in detail (4) and a single illustration will serve for the present thesis. The right precentral gyrus was destroyed by cauterization in a cebus monkey. A severe paralysis of the left arm and leg followed. The animal was then trained to open a variety of latch boxes with his right hand. During the training there was some recovery from the paralysis but the left arm and hand were used only as a stiff prop to support the body during the manipulations with the right hand and at no time during training did the animal grasp at the latches with his left hand. When the habits were fixed the left precentral gyrus was destroyed, with a resultant paralysis of the trained right hand. The animal was kept without further training until the paralysis of the right hand had so far recovered as to seem to permit of manipulation of the latches again. His ability to open the boxes was then tested. In the meantime he had acquired facility in the use of his left hand and the right was now by preference used only as a prop.

When confronted with the problem boxes he fumbled clumsily at the catches of each during a few trials with his right hand, then attacked the fastenings with his left hand and released them *without random movements* and almost as quickly as he had formerly done with his right hand after protracted training.

There was almost perfect transfer of the habit to the hand which had been paralyzed during training.

Here, as in the centripetal end of the arc, we have evidence of the utilization of neural paths in the performance of a learned reaction which were not activated during the course of learning. In this case the drainage theory is definitely ruled out. Irradiation of impulses to symmetrically placed spinal neurons might account for the results but I have records of other cases where the head or leg was substituted for the arm rendered useless by operation and where, consequently, irradiation cannot explain the transfer of the habit to the unused organ.

It seems certain, then, that we have here two types of habit formation which cannot be explained by any wearing down of synaptic resistance through the passage of nerve impulses. The behavior is of a complex character, approximating what is termed ideational behavior in man. Placing it in this category, however, does not advance our problem, which is to understand the physiology of reintegration. Most familiar cases of learning, even the so-called reflex, approach this order of complexity so that these cannot be looked upon as exceptional cases but rather tend to discredit the theory as applied to any learning.

An alternative theory is not possible in the present state of the science of neurophysiology but there are suggestions in recent experimental work that such a theory must develop in a quite different direction from that which it has thus far taken. Spinal reflexes as they are usually described are such simple diagrammatic affairs that they have captured our imagination to the exclusion of other neurological phenomena and have been taken as the pattern of all nervous activity. As conditioned reflexes they have been expanded to embrace all nervous activity, until the telephone system has come to be looked upon as an adequate analogy for all central nervous function. But the phenomena of equipotentiality of function of cerebral areas (5), of vicarious function without definite localization (6), of mass action of central ganglia (7), of temporal variation in function (8), and the accumulating mass of evidence which indicates that in the intact organism every reaction is an expression of the total activity of the central nervous system, all are difficult to harmonize with a system of anatomically restricted reflex paths defined by varying degrees of synaptic resistance.

Work upon the "all or nothing" character of conduction and the refractory period of nerve suggests that the rate at which the neuron may transmit successive impulses is at least as important in determining the direction of conduction as the degree of synaptic resistance. The theories of inhibition of Verworn (9) and Lucas (10) involve the conception of critical frequencies of impulses for each neuron. If this should be verified for the central nervous system we may have to seek for changes in the refractory period rather than at the synapse as the basis of the learning process and for changes in the periodicity of discharge within a syncytium rather than for anatomically defined conduction paths to account for new integrations.

The observations recorded above are more in accord with such an hypothesis than with the current one based upon synaptic resistance, but detailed speculation in this field is futile so long as experimental work is unavailable. I am concerned here only to point out the inadequacy of present learning theory and to emphasize the importance of neurological investigation for the correct interpretation of even such psychological findings as the "Law of Frequency" in learning.

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GLOSSARY

Afferent Tactile Fibers, fibers from cutaneous receptors, conducting toward the brain.

Afferent Tract, tract conducting toward the brain.

All or None Principle, the principle that an organ functions at a given level of activity or not at all.

Anastomose, join, fuse, entangle.

Cauterization, destruction by burning or searing.

Centrifugal, leading or conducting outward toward the periphery or outer margins of the body; away from the center; efferent.

Centripetal, leading or conducting inward, toward the center (the brain); toward the center; afferent.

Contralateral, on the opposite side of (the body or brain).

Effector Mechanisms, nerve mechanisms that connect the brain with the muscles, innervating the latter.

Equipotentiality, balanced condition; a field property; equal possibility of function.

Ganglion Cells, cells located in ganglia or masses of cell bodies; pertains here to semi-independent nerve centers.

Homolateral, on the same side of (the body or brain).

Motor Neurons, neurons conducting from the brain to the muscle; effector mechanisms.

Photic, visual; pertaining to light.

Precentral Gyrus, fold (convolution) in front of the precentral fissure, a deep fold toward the front or anterior, upper portion of the cortex; location of so-called motor area.

Precortical Level, level of the brain reached by incoming impulses before the cortex is reached.

Spinal Neurons, neurons in the spinal cord.

Striate Areas, furrowed or striped areas; (as used) certain regions of the occipital cortex.

Subcortical Centrifugal Level, region beneath the cortex that sends impulses to the muscles.

Syncytium, tissue without recognizable cell boundaries.

READING TWENTY-SEVEN[1]

THE EARLY DEVELOPMENT OF BEHAVIOR IN *AMBLYSTOMA* AND IN MAN[2]

Editor's Note

Eight Organismic Laws Applied to the Nervous System.—A study of the nervous system is the last step to be taken in examining the conditions of behavior. The Readings began with a study of the human being in his natural, unrestricted environment, with no systematic limitation of his behavior. Gradually this behavior was limited by a restriction of its conditions until only the most specialized and the simplest types of response remained for investigation. These were sensory processes. The last step removes from direct observation any mode of conscious behavior and reduces the organism to a purely physiological creature. Accordingly, the physiological conditions of behavior are now the direct objects of study, while the conscious aspects of behavior are taken for granted.

Throughout the Readings, the organismic nature of all forms of behavior was evident. The same principles underlie the functioning of the nervous system, and here they are even more apparent because methods of investigation are more exact, and predictions are more certain. For convenience, the eight organismic laws given in Reading One can be recalled again, now in relation to the nervous system itself.

First, that the whole is more than the sum of its parts is borne out by various lines of evidence. Once the nervous system has reached a certain stage of growth and differentiation, an isolated part will not carry on the functions of the whole, nor will the specialized activities of these parts combine or synthesize of themselves into a co-ordinated performance. For example, remove the cortex, and the animal's behavior is greatly restricted. Reflexes, or stereotyped and relatively undirected movements of different muscle groups, will occur, but these movements do not integrate. Reflexes of the legs will not summate to produce the more complicated goal-activities of the intact animal. A frog, thus reduced to a reflex animal, will remain in a pan of water slowly heated to boiling; a bird, similarly reduced, will fly if placed in a total situation demanding it, as when thrown into the air, but it will alight on a hot perch, lifting first one foot and then the other without a co-ordinated effort to save itself from being burned. During the embryonic growth of the nervous system, reflexes which develop in particular muscle-groups such as the arms, legs, and trunk do not integrate after the fashion required by the conditioned reflex theory; they do not integrate at all. The movements of the organism as a whole are properties peculiar to the whole, over and above the properties that are exhibited by the parts. This is the *law of field properties* again.

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[1] Parallels *The Science of Psychology,* Chapter XVII.

[2] By G. E. Coghill, Wistar Institute, Philadelphia. From the *Archives of Neurology and Psychiatry,* Vol. 21, 1929, 989-1009. By permission. (Certain references and the tables omitted.)

Second, the parts derive their properties from the whole. Coghill and others have recently shown that the motility of various parts of the body is acquired *only* through the relations of those parts to the body as a whole. The first movements of the fore limbs in an embryo are imparted to them by the expansion of a total pattern of movement so that at first they move only with the body. The same facts hold for other parts of the organism, thus demonstrating the *law of derived properties.*

Third, the whole conditions the activities of its parts. The precise reasons for this fact can be found when the embryonic development of the nervous system is carefully studied, as Professor Coghill has done. A unified total pattern of nerve structures develops throughout the organism. In this growth process the development of appendages, or any muscle-groups that are later to function *relatively* independently, is anticipated by the total pattern, for the total pattern sends out branches that are ready to connect with the muscle-groups when the latter are mature enough to function. More specifically, the same neurons that connect with trunk-muscles send branches or collaterals toward the developing limbs.

But this is by no means the whole story. At every stage in the growth of the embryo from a single cell on, the whole governs the activities of its parts. In the single cell, there are gradients of metabolic activity which have been described in detail by Child, Coghill, and others. By gradient of metabolic activity is meant a differential in the rate of living, an alignment of growth and activity potentials. A gradient is a field property. In other words, the single cell is an integration of metabolic potentials. These gradients determine when and where the various organs of the body shall develop and what their functions shall be; they determine the direction in which fibers of the nervous system lengthen out in their process of connecting remote parts of the body with the brain and spinal cord; they determine the polarity of the neuron, that is, the end that shall receive and the end that shall discharge nerve impulses. At each step in the growth process, new relationships arise between the parts through their acquired relationships to the total pattern. These new relationships, in turn, condition the manner in which the parts continue to be differentiated until they constitute relatively segregated wholes. But the independence of parts is never complete. The *law of determined action* is thus borne out.

Fourth, parts emerge from the whole through an individuation process. Coghill has beautifully demonstrated this fact in the growth of the nervous system in salamander embryos. Not only has he watched the movements of parts differentiate out from the movement of the whole, but he has traced the process back to the anatomical development of the underlying nerve patterns. The first movements of the embryo are of the total organism. General movements of the legs, head, and segments of the trunk emerge first, but these are themselves undifferentiated; the legs move as a whole; the head moves as a whole, and so on. Consider, now, the development of specialized movements within a leg-movement, say the fore leg. Separate movements of the upper and lower parts of the leg, with the use of the elbow as a joint, emerge first; total "hand" movements emerge from the fore arm movements; total finger movements emerge from the hand movement; and finally, movements of separate joints of the fingers emerge from total finger movements.

The same type of development takes place in the head-region. The salamander embryo, for example, first moves the head as a whole, *with* shoulder and fore leg movements. It will lunge toward a morsel of food (fly) before it can move its jaws separately from the head as a whole. A little later, jaw movements emerge from the total head-movement. Now the animal will snap at the fly, and will even hold it in its mouth, but there are as yet no swallowing movements. Tongue and swallowing movements are the last to appear in the gradually differentiating total pattern. (The *law of individuation.*)

Fifth, wholes evolve as wholes. Throughout the development of the nervous system the total pattern maturates as a dynamic unit; each smaller pattern, emerging from the whole, develops, also, as a dynamic unit, never losing its membership in the total pattern from which it differentiates. Nowhere, from the time the embryo is a single cell to the time it has reached physical "maturity" is there a building up of a complex pattern from units such as cells or reflexes. Organization exists prior to the multiplication of cells and prior to the appearance of reflexes; it conditions the appearance of both. The whole is therefore primary in a genetic as well as in a logical sense, and the parts must be explained in terms of the whole—the "lower" in terms of the "higher." Embryonic growth illustrates the *law of field genesis.*

Sixth, the law of least action, as defined in the Readings, is illustrated throughout the growth and functioning of the nervous system. Differentials in potential determine the position and direction of growth of the various organs of the body. The polarity of the neurons, and the exact position in the muscle cell where the motor neuron will connect with it, are conditioned in a similar fashion.

Seventh, that energy systems resist disintegration is demonstrated at the level of living organisms by the fact that if the organism as a whole is disequilibrated by external agencies, it proceeds at once to regain, if possible, its balance of potentials. Take an embryo in a sufficiently primitive state and exchange tissue in the head region with tissue in the tail region. Then the tissue that originally would have grown into a head develops into a tail, and the tissue that would have grown into a tail develops into a head. This is caused by the fact that the metabolic gradient extending the length of the organism, with its highest rate at one end and lowest rate at the other, determines the position and character of the organs that will develop all along the gradient. The largest, more specialized and complicated structures of any particular type, develop in the regions of high metabolic rate. Exchanging the tissues momentarily distorts the gradient but if a sufficient gradient remains, the tissues will assume their new rôle by adjusting their metabolic rates accordingly.

The same law is illustrated in the healing of injuries, and in the regrowth processes that take place where tissue is destroyed. It is illustrated in any kind of "work" performed by the organism in adjusting itself to destructive influences from outside. In all of these adjustments and likewise in disease, whether "mental" or "physical," the organism's energies are bent to the fullest extent possible, under the existing conditions, toward the establishment of a balance. In other words, the physiological activities of the organism are following the *law of maximum work.*

Eighth, the law of configuration is not only demonstrated in the growth and functioning of the nervous system, but also in growth processes that occur before signs of a nervous system appear. The gradients characteristic of single cells are conditioned by total pressure-patterns exerted upon the cells from the environmental medium in which they live. Alter these patterns, and the organism as a whole adjusts its growth processes accordingly. In the single cell, for example, the surface-interior gradient begins at the surface with its highest rate of metabolism, and declines in the direction of the center. The highest metabolic rate, therefore, exists where irritation or stimulation from pressure-patterns outside is the greatest. Cut this single cell in two and the cut surface takes on the metabolic properties of an external surface. A new organism comes into being during the formation of a new surface-interior gradient. Finally, bring excessive irritation or pressure to bear upon parts of the embryo that ordinarily are not subjected to such pressures, and monstrosities of growth are produced in the form of tumors, or extra legs, or heads.

The growth of parts within the whole is throughout a process of reaching remote ends established by pre-existing gradients. The same sets of conditions that provide the high metabolic rates provide the low. Growth will not take place until the conditions are set for its completion. These conditions are furnished, first, by the adjustments of a total organism to a total situation surrounding it, and second, by the adjustments of parts, in the form of relatively segregated wholes, to the total situation which surrounds them, namely, the pattern of which the parts are members.

In *Amblystoma,* a genus of the amphibian *Urodela,* one finds an enlightening approach to the study of behavior. Unlike the *Anura* (frogs, toads, etc.), *Amblystoma* does not resorb the tail in metamorphosis from the tadpole or larval stage to the adult condition. The trunk and tail, on the other hand, continue throughout life as a flexible, integrated organ, the bending of which gives the animal its power of aquatic locomotion and, subsequently, becomes the primary component of locomotion on land. This lack of anatomic and physiologic specialization in the axial organization of *Amblystoma* makes the animal peculiarly adapted to the study of the development of behavior, and I have taken advantage of this adaptation by observing the behavior of different species of *Amblystoma* from the earliest movements till the general adult pattern of action is established.

As a result of this investigation of the development of behavior a general principle has been discovered which, in my opinion, has wide, if not universal, application in vertebrates. The principle may be stated thus: The behavior pattern from the beginning expands throughout the growing normal animal as a perfectly integrated unit, whereas partial patterns arise within the total pattern and, by a process of individuation, acquire secondarily varying degrees of independence. According to this principle, such an entity as a ''simple reflex'' never occurs in the life of the individual; complexity of behavior is not derived by progressive integration of more and more originally discrete units; the conception of chain reflexes as usually presented is not in accord with the actual working of the nervous system. On the other hand, within the total, ever expanding integrated organism as a whole, partial patterns emerge more or less and tend toward independence and dominance, but, under normal conditions, always remain under the supremacy of the individual as a whole. An inappropriate degree of independence or dominance of a partial pattern constitutes abnormality or perversion of behavior.

The evidence on which this interpretation is founded cannot be given in great detail here, but a statement of some of the more important items is essential to the purpose of this paper, which is to show that, so far as the early fetal movements of man are known, the same law prevails in the development of behavior in the human being as that which has been observed in *Amblystoma.*

THE DEVELOPMENT OF BEHAVIOR IN *Amblystoma*

The earliest movements of *Amblystoma* are made by the most anterior part of the muscle system of the trunk. They consist of a bending of that part of the trunk to one side or the other. As the animal grows older, this process of bending (flexure) extends farther and farther tailward until the whole segmental muscular system, trunk and tail, is involved. During the earlier period, all movements begin in the anterior region and progress tailward. This phase of development, therefore, consists of the expansion of a totally integrated pattern as a whole, and there are in it as yet no discretely individuated components.

These movements are at first relatively slow, but they increase in speed as the animal grows older, and, with this acceleration, the movements acquire locomotor value because each bend of the body progresses from the head tailward. In the earlier period, and still for a time after the whole trunk has become involved in the performance, the flexures are simple, that is to say, the body, in any particular act, bends only to the right or to the left. But as a result of still further increase in speed of action a second flexure in the opposite direction to that of the first in the anterior part of the body begins before the first flexure has passed tailward completely through the trunk. A series of such rapid flexures, alternating in first one and then the other direction, produces swimming. Locomotion in the water arises, therefore, by the expansion and speeding up of the original and primary pattern of behavior.

A merely casual observation of the earliest movements of *Amblystoma* in response to tactile stimulation would probably give the impression that they are simple reflexes of the conventional type; but there are several valid objections to such an interpretation. Both the afferent and the efferent paths consist of several or many neurons. Anatomically, therefore, the mechanism at the basis of these reactions differs materially from the conventionally adopted mechanism of the simple reflex. Furthermore, in their early stages of differentiation from neuroblasts and in their earliest functional period, the motor centers of the two sides constitute, in the region of the head, a single center that spreads across the floor plate or median line of the brain as a unitary structure, so that half-centers, according to Graham Brown's conception, do not exist as separate entities in *Amblystoma* of this early stage.

Physiologically considered, the flexure to the right, for example, might be regarded as antagonistic to flexure to the left, and on that basis the development of the swimming movement might be interpreted as a process of integration of primarily antagonistic reflexes, or the resolution of primarily discrete reflexes into chain reflexes. The latter interpretation seemed to me adequate at one time; but it has not yet been demonstrated that the sensory part of the conduction circuit is necessary for swimming in *Amblystoma;* whereas Tracy has shown that movements that are essentially like the swimming movement of *Amblystoma* occur in the toadfish before there is a functional exteroceptive or proprioceptive sensory field to participate in the reactions. In other words, movements such as constitute swimming in *Amblystoma* can be organized wholly within the motor mechanism of the nervous system. Furthermore, although the right and left flexures would appear to be potentially antagonistic to each other, I have found no evidence of actual antagonism between them in the normal development of swimming. As speed of movement accelerates, provision is made against such antagonism by the growth of collaterals from the motor neurons of each side into such relations with the motor neurons of the other as to produce inhibition on the side opposite a prevailing excitation. Here again, it should be emphasized that the mechanism is wholly on the motor side of the conduction circuit; the sensory system has no perceptible part in this inhibitory mechanism. But this growth of collaterals does not bring originally isolated motor centers into a new relation, for the motor mechanism of *Amblystoma,* as already mentioned, is originally unitary and median as regards the two halves of the body. These collaterals, developing simultaneously with the differentiation of the common unitary center into bilateral half-centers, serve only to keep the whole motor mechanism in co-ordination. It has never been unco-ordinated. On the whole, therefore, the evidence is against interpretation of swimming movements of *Amblystoma* as chain reflexes. It is, on the other hand, the expression of an expanding total pattern that has been perfectly integrated from the first.

When *Amblystoma* begins to swim, it has no motile appendages. As the external gills, fore limbs and hind limbs develop, their earliest movements are performed only as the trunk moves. The total behavior pattern, in other words, expands into the appendages as a primarily integrated system. This is true both for spontaneous movements and for movements in response to stimulation. This system of total integration dominates the appendages for a considerable time before they acquire any perceptible independence of action; that is to say, before local reflexes emerge from the total pattern by a process of individuation. And it is important to note that the early phases of this emergence of discrete limb movement are characterized by a gradual reduction of the trunk component of the total pattern of trunk movement plus limb movement. The same principle holds also for the distal segments of the limbs: the earliest elbow or knee flexion occurs with movement of the limb as a whole.

Furthermore, the primary integration of the four limbs with the trunk dominates the early movements of walking. The primary component of walking is a slow swimming movement of the trunk and tail, which involves the limbs. The fore limbs are integrated with the movement of the trunk while the movement affects the anterior part, and as the trunk movement approaches the region of the hind limbs they also become involved. Accordingly, the hind limbs do not move synchronously with the fore limbs, but lag behind them by the interval required for the trunk flexure to pass from the region of the fore limb to that of the hind limb. The co-ordination of the limbs in the walking gait is therefore not a matter of nervous co-ordination directly between the limbs, but an integration of the limbs directly with the trunk. In its early phases of development, then, walking in *Amblystoma* is not a function of chain reflexes; it is a total pattern. Within this total pattern special adaptations in movement may eventually arise by the individuation of local reflexes. The antigravity component is probably of this nature.

Since, as shown elsewhere, the capture and swallowing of food obviously arises according to the same mode of development as just outlined for locomotion, it may be said that the behavior of *Amblystoma*, so far as the form of the pattern is concerned, develops, not by the integration of simple reflexes into chains, but by the expansion of an integrated total pattern and an individuation of partial patterns of more or less local character which normally acquire a greater or less degree of independence as occasion requires and as is consistent with the integrity of the individual as a whole.

Available Evidence on the Origin of Reflexes in Man

The movements of the human fetus in utero have attracted attention for many years, but direct observation which permitted accurate description of the form of movement of young fetuses has been for the most part casual and fragmentary, except the researches of Minkowski and of Bolaffio and Artom. The studies of Minkowski have been continued consistently through a number of years, and his observations are remarkable for their detail and scope; those of Bolaffio and Artom, while less extensive than Minkowski's, are recent, and add materially to the knowledge on the subject. The total number of human fetuses under 6 months which are recorded in such a manner as to make them useful for this study stands at a minimum of fifty.

Beyond the age of 6 months, human fetuses have been studied extensively, particularly by Minkowski, Bersot and Bolaffio and Artom. The behavior of these older fetuses is important for the study of the development of reflexes, but it is to the younger stages that one must look for the mode of origin of reflex behavior.

SPONTANEOUS MOVEMENT

The earliest spontaneous movement that has been observed in man is recorded by Yanase for a fetus of crown-rump length of 2 cm. In this case the right arm moved back and forth about six or seven times; but Yanase considers that in the considerable time that elapsed after the fetus was removed and before it came under his observation, it may have made movements "in toto." In a specimen of 2.2 cm., Strassmann observed repeated movements of the arms and legs, whereas Minkowski describes the earliest spontaneous movements as vermicular and involving the body, head, arms and legs.

In the fetus 2 cm. in length which he observed, Yanase says that the fingers were separated only at the tips. In the specimen of 8.5 cm. Minkowski says there were isolated finger movements with trembling of the thumb. Presumably he means by "isolated" that the fingers moved independently of any movements in the arms or hands, that is to say, the fingers and thumbs all moved in a common performance. Minkowski reports spontaneous finger movements also in a specimen of 20 cm., and in another of 25 cm. In the latter he saw also spontaneous movements of the toes.

Beginning with Minkowski's specimen of 4 cm., in which there was slow vermicular movement of the head, trunk, arms and legs, the behavior pattern as indicated by spontaneous movement obviously expands through the extremities, as it does in *Amblystoma.* But in the two earlier specimens of 2 cm. and 2.2 cm. only limb movements were observed. On this evidence alone, therefore, it might appear that the axial musculature has not the precedence to and dominance over limb musculature in the human fetus that it has in *Amblystoma.* But in the evaluation of this evidence, at least three considerations must be kept in mind: (1) the massive nature of the head of the human fetus would offer relatively much greater resistance to the contractions of axial musculature in man than in *Amblystoma;* (2) there is the possibility of trunk movements being unobserved, as Yanase explicitly indicates; (3) there are only two cases on record in which the fetuses were younger than Minkowski's specimen of 4 cm. that exhibited vermicular movements of head, trunk, arms and legs.

REFLEXES

Among the earlier reflexes many have an extensive pattern: mouth movements with leg movements (3.5 cm.); simultaneous symmetrical movements of shoulder, arm, legs (5.5 cm.); arms and legs (5.7 cm.); grasping movement of toes (6.5 cm.); contraction of musculus orbicularis palpebræ (oculi) with action of both legs (6.5 cm.); patellar reflex with action of opposite leg and both arms (7 cm.); flexion, adduction and rotation of the arm with movement of the other arm and both legs and rotation of the head in response to mechanical stimulus on the arm (7 cm.). During the corresponding period, however, certain localized reflexes have been recorded: patellar reflex in extension at knee (6.5 cm.); leg flexion (7 cm.); thorax reflex (5.7 cm.), and abdominal reflex (7 cm.). The interrelation of these phenomena requires special consideration.

The Oral Reflex. The earliest oral reflex in man is recorded by Minkowski for a fetus of 3.5 cm. But the opening and closing of the mouth in this case did not occur as a discrete act; it was allied with leg movement; that is to say, the reflex pattern involved the neuro-motor mechanism from the trigeminal nuclei to the distal spinal centers. On the other hand, the earliest opening and closing of the mouth as a pure local reflex, that is, without participation of other parts, is recorded explicitly first at 21.5 cm. So far as my evidence goes, therefore, the oral movements in response to touch on or near the mouth occur at first as a component of a much larger pattern of movement and only later appear as a discrete reflex.

Limb Reflexes. Isolated extension of the knee occurs at 6.5 cm. but the first reflex movement of the leg occurs with action of the other leg, of both shoulders and of both arms at 5.5 cm., and in alliance with the arms only at 5.7 cm. Minkowski records the patellar reflex in alliance with action of the other leg and both arms at 7 cm., whereas he apparently observed a pure patellar reflex at 6.5 cm. Authors acknowledge, however, that the tendon reflexes are difficult to make sure of in the early stages of fetal life, and this possible slight precedence of isolated patellar to the general patellar reflex pattern should not receive great emphasis.

Discrete reflex of the arm is first recorded at 7 cm., whereas the arm participated in a reflex pattern which included the legs at 5.7 cm., and in a pattern that included the other arm, the shoulders and the legs at 5.5 cm. This earliest discrete reflex movement of the arm at 7 cm. involved both adduction and rotation, and was therefore not a simple reflex; it was a relatively complex or extensive pattern as regards the arm itself.

But still, at 7 cm. the extensive reflex pattern, including arms, legs and head, amounting almost to a total reaction, can be elicited. This particular reaction, observed by Bolaffio and Artom, seems to me to be of great importance, for at 9 cm. the same authors observed movement of both arms in response to touch on one, and response of both legs to touch on one. Nevertheless, they observed a reflex movement of one leg only at 7 cm. There is evidently a tendency here for the total pattern of which the discrete reflex is an organic component to recur in greater or less degree after the discrete reflex has become individuated. This principle is observed particularly by Minkowski and Bersot in their study of later fetal and postnatal reflexes.

On the basis of reflex movements alone, therefore, it appears that discrete movement of the arm or leg does not arise as such, but emerges from a general pattern that involves other appendages, and probably more or less of the axial system.

But it will be recalled that the earliest observed spontaneous movement (at 2 cm.) was movement of the one arm only, and this might be regarded as contradictory to the conclusion just stated. But the observer, as previously noted, considered that in this case more extensive movements may have occurred. Moreover, the capacity for movement is not as completely expressed spontaneously as it is under excitation, for Bersot explicitly states that reflexes are elicitable in human fetuses after spontaneous movements cease, and the same feature is obvious in Minkowski's records. Furthermore, at 2.2 cm., the arms and legs participated in spontaneous movement. The difference in length of 0.2 cm. between the two specimens under consideration is probably negligible as regards chronologic order, for it may easily be due to differences in methods of measurement or to the manipulation of the specimen. This early spontaneous movement of the arm alone, therefore, does not seem to me to be necessarily antagonistic to the conclusion I have drawn from reflex movements of the appendages, that discrete movement of a limb emerges from a larger pattern of integration.

The Plantar Reflex. The earliest recorded form of reflex in response to stimulation of the sole of the foot occurred in a fetus of 7.5 cm. It consisted of extension of the leg and dorsal flexion of the foot without positive movement of the toes. In a specimen of 8.5 cm. Minkowski says that response was plantar flexion with rotation of the foot and ''spontaneous inversion'' of the reflex type. Minkowski does not say here explicitly that the toes moved when the foot moved, but he does say that in response to tactile stimulation of the sole of the foot there occurred movement of the foot outward with finally a plantar flexion, and that there occurred a ''spontaneous inversion'' of the reflex type, whereas in the context he defines ''spontaneous inversion'' as applying explicitly to toe movements. It seems certain, therefore, that the earliest movement of the toes that Minkowski observed as occurring in response to stimulation of the sole of the foot in his study of the development of the plantar reflex occurred in the same reaction with movements of the foot. This was at 8.5 cm., whereas the earliest movement of the toes that he observed without movements of the foot or leg occurred at 10 cm. It appears, then, that one has a complete history of the individuation of the pattern of movement of the toes as a group out of the larger pattern of movement of the leg, foot and toes.

It is significant also that the earliest response of the foot to plantar stimulation (8.5 cm.) is not a simple movement; it is flexion and rotation. Similarly, the earliest movement of the toes, recorded without mention of the foot or leg, is the grasping reflex (not to plantar stimulation), which is an action of all the toes together, a total reaction of the toes. This occurs at 6.5 cm. (Minkowski). But it is not until the fetus reaches 10 cm. in length that the plantar reflexes involve the toes only, and in this case Minkowski says that the toe movements were weak. In the fetus of 10.5 cm. there occur dorsal flexion of all the toes and spreading of the toes, the foot and leg not being mentioned. This reaction, in my opinion, may be regarded as a total reaction of the toes before discrete reaction of any particular toe occurs. Accordingly, in his 13 cm. fetus, Minkowski records what, in my judgment, is a total reaction of leg and toes (dorsal flexion and spreading of all the toes and flexion of the leg), whereas in a 14 cm. stage he saw spreading of the toes explicitly without flexion of the toes or movements of the foot or leg. Here, according to my interpretation, spreading of the toes has become individuated as a local reflex within the total leg-foot-toe system as represented in the 13 cm. stage.

In a specimen of the 16 cm. stage, Minkowski caught a phase of the development of the plantar reflex which has especial importance: in a total foot-flexion, toe-flexion and toe-spreading reaction, the flexion of the great toe was stronger than that of the other toes. In other words, discrete flexion of the great toe was here just beginning to emerge from the general or total system of toe movement. Accordingly, in the other 16 cm. specimen plantar flexion of the small toes occurred without participation of the great toe. Presumably, this isolation occurred because of the degree of individuation that the great toe had acquired for dorsal flexion, or, in other words inhibition is an essential factor in the process of individuation of the reflex. This interpretation is corroborated by Minkowski's explicit statement that the plantar flexion of the toes in one specimen (16 cm.) was a weak movement. It is further corroborated by another specimen, a 17.5 cm. fetus, which in the first reaction gave a total toe-dorsal-flexion and spreading reaction and in the second reaction gave plantar flexion of the small toes only.

In the 18 cm. fetus complete individuation of the mechanism of the great toe is functionally perfect in the second response, which consists of dorsal flexion of the great toe with plantar flexion of the small toes; but in the third response, possibly due to fatigue, the great toe falls back into the total system of toe-plantar-flexion. In two 20 cm. and one 21.5 cm. specimens the total toe pattern prevails in the form of dorsal flexion and spreading for the most part, but in one of the 20 cm. specimens, in some of its reactions, the great toe emerges from the total pattern in a discrete dorsal flexion.

For the 21.5 cm. fetus Minkowski records explicitly the first typical isolated Babinski reflex in his protocols, but in his discussion he refers to the 18 cm. fetus as giving the earliest form of the Babinski sign. According to either of these statements, this typical reflex, according to the analysis presented here, has clearly arisen by a process of individuation within a total leg-foot-toe response.

The plantar reflexes of fetuses of 22 to 32 cm., as recorded by Minkowski, show frequent reversion to the earlier types of response through which the Babinski reflex developed. This phenomenon has been noted previously in relation to the limb reflexes.

The Eyelid Reflex. The earliest observed contraction of the musculus orbicularis palpebræ (oculi) occurred with action of both legs at 6.5 cm., whereas discrete reflex action of this muscle occurred first at 21.5 cm., both observations being by Minkowski.

The Anal Reflex. The earliest response to stimulation of the anal mucous membrane involved strong actions of both legs with contraction of the musculus constrictor ani. This was at 13 cm. and no isolated anal reflex is recorded for fetuses under 7 months. It appears, therefore, that this reflex also makes its appearance as a component of a larger reflex pattern.

Respiratory Reflexes. The earliest movements which were explicitly designated as respiratory by the observer (Erbkam, 16 cm.) involved opening of the mouth, movements of the head from side to side and movements of the arms and legs. In a fetus of 21.5 cm. Minkowski describes the respiratory movements as convulsive. These observations indicate that the earliest respiratory efforts are virtually total reactions of the fetus. Nevertheless, reflex movements of the thorax are recorded for a fetus of 5.7 cm. and the abdominal reflex for one of 7 cm. For the latter age, spontaneous contractions of the intercostal muscles are also recorded (Bolaffio and Artom); however, these movements may have been direct responses of the muscles that were involved, for the muscles lie immediately beneath the skin and the stimulus was percussion or stroking. Furthermore, it is not clear that the reflex of the thorax observed by Yanase in the fetus of 5.7 cm. was not a direct response of the muscle to electrical stimulation, for he used electrical stimulation on this specimen and electrolytes may have been carried from the solutions on the fingers or instruments. Also the abdominal reflex observed by Bolaffio and Artom is open to question, for these authors used percussion on the abdominal wall in this instance. Although the reaction was bilateral, the mechanical impact on one side may have extended through the aponeurosis to the muscle of the opposite side. This interpretation is corroborated by the fact that to percussion on one side of the pectoral region, where the mechanical construction of the body is more rigid and the muscle masses of the two sides are more widely separated than in the abdominal region, Bolaffio and Artom could elicit only homolateral responses of the pectoral muscles in this same specimen. Furthermore, in a decerebrated fetus of 7 cm. they observed that the thigh participated with the abdominal muscles in a reaction to a unilateral excitation over the ribs. This would indicate that there is very early alliance of the limbs with action of the abdominal muscles, and that in all probability the abdominal reflex arises by individuation out of a larger pattern of integration or that it does not arise as a simple reflex and later become integrated with other reflexes into the respiratory reflex.

Having given arm movement (and probably trunk movement) at 2 cm., integrated arm and leg movement at 2.2 cm., and mouth movement integrated with movements of legs and arms (and probably trunk, for Minkowski describes the movements of 4 cm. fetuses as vermicular, involving head, trunk and limbs) at 3.5 cm., it appears that this expanding pattern invades the intercostal and abdominal muscles by the time the fetus reaches 7 cm. in length, at which age Bolaffio and Artom saw spontaneous contraction of the intercostal muscles and reflex contraction of the abdominal muscles (providing their interpretation of the abdominal reflex at that age is valid), and that this total pattern acquires the vigor of action observed by Erbkam at 16 cm.; whereas the definitive respiratory pattern becomes individuated from the total pattern at about the 28 cm. stage as observed by Bolaffio and Artom, or by the 30 cm. stage as observed by Minkowski. At any rate, I fail to find in the records of the behavior of human fetuses any dependable evidence that the definite respiratory movement is acquired by the integration of discrete simple reflexes or by the development of chain reflexes. On the other hand, the evidence seems clear to me that this movement is acquired through the expansion of the integrated total pattern of movement as a whole, and by individuation within that pattern of the particular integrated system that the integrity of the organism requires as development proceeds.

BERSOT'S INTERPRETATION OF THE PLANTAR REFLEX

It is held by Bersot that the earliest response to stimulation of the sole of the foot involves the toes only and that it consists of ventral flexion of the proximal phalanges and dorsal flexion (extension) of the others. These toe movements, Bersot says elsewhere, are slow and weak but clear, and involve especially the four small toes, while the great toe moves with difficulty and more often remains entirely immobile. The toes, he says, seem to stretch out; they become fatigued rapidly and cease to respond after six or eight reactions. Only later in development, according to Bersot, is the limb withdrawn from the stimulus on the sole, with extension of the opposite limb, and eventually, the reaction becoming more and more generalized, the entire organism participates in the motor reaction. Finally, that is, at from 2 to 6 months of postnatal life, the reaction again restricts itself to narrower limits, and acquires the pattern of the definitive plantar reflex.

According to Bersot's interpretation, then, the reflex, of which the plantar may be regarded as typical, involves, when it is first performed, only the local part that is stimulated; that is to say, it is a local reflex, pure and simple, and only in later development progressively diffuses or irradiates more and more extensively until practically the whole organism participates in the reaction; and, finally, this process is reversed, and there takes place a localization of the response to the extent that only the local stimulated part may react to the appropriate stimulus. This theory of the development of the reflex, as proposed by Bersot, is explicitly adopted also by Minkowski.

It appears that the later phase of the development of the plantar reflex, the phase of concentration, according to Bersot and Minkowski, is in agreement with my observation of the process which I call individuation in the development of reflexes in *Amblystoma.* The interpretation, however, that the plantar reflex begins with movement of the toes only, appears not to be in harmony either with my observation on *Amblystoma,* or with certain observations of Minkowski on the human fetus.

Minkowski, in describing the behavior of four fetuses of from 5 to 5.5 cm. total length, as already noted, says that they performed cutaneous and proprioceptive reflexes, short and long, homolateral and crossed. Most often, he says, it was not a question of isolated reflexes, but of general motor reactions that were variable. Excitation of the foot, for instance, could provoke, besides a reaction of the stimulated leg, motor reactions in the two arms, the head and the trunk. Minkowski does not say that this stimulus on the foot might be on the sole, but in another place he demonstrates that the plantar reflex as late as the 22 cm. stage is provokable by stimulation on the dorsum as well as on the plantar surface of the foot.

According to this principle, which is emphasized by both Bersot and Minkowski, that in the early stages of the development of reflexes there is no well defined localized specificity of the receptor field for the excitation of a particular reaction, the extensive reactions which Minkowski describes as involving movement of the trunk, head, arms and legs in response to stimulation on the foot in fetuses of 5 cm. in length may well be regarded as related to the local reaction of the foot and toes in exactly the same manner as the total reaction of the trunk and limb of *Amblystoma* is related to the local reflex of the limb. Furthermore, Minkowski records observations on a fetus of 7.5 cm. in length that moved the foot and leg in response to stimulation on the sole of the foot. In this case the locus of the stimulus was specific for the plantar reflex, and, although movements of the toes were not observed, this reaction may well be regarded as a precursor of the typical plantar reflex of the toes, that is to say, a phase of the individuation of the reflex out of the total pattern, which the more extensive reactions just mentioned for 5 cm. fetuses may be considered to represent. The validity of Bersot's interpretation of the mode of origin of the reflex depends on whether the early movements of the human fetus lack integration.

INTEGRATION OR DIFFUSION IN THE EARLY MOVEMENTS OF THE HUMAN FETUS

In his report on a group of fetuses ranging from 5 to 23 cm. in length, Minkowski says that the head turned from side to side, was elevated and depressed; the trunk curved and straightened, and the extremities flexed, extended and rotated. Minkowski does not make it certain that all the components of the behavior pattern, or any particular number of them, were operative in any given performance except that in some cases several articulations were involved simultaneously. He describes these movements, however, as asymmetrical, arrhythmic, diffuse and inco-ordinate. Nevertheless, he says in the context that one of these reactions may apply to more than one limb simultaneously. This would seem to contradict his statement that the movements are inco-ordinated. In another contribution he says that spontaneous movements of four fetuses of from 5 to 5.5 cm. in length involve rotation, raising and lowering of the head; flexion and extension of the trunk; rotation, abduction and adduction, flexion and extension at the elbow, shoulder, wrist and hip, and that movements at several articulations take place simultaneously. He says also that excitation of the foot provoked, besides reaction of the excited leg, motor reactions in the two arms, the head and the trunk.

Furthermore, Minkowski says elsewhere that the crossed reflex shows the first elements of co-ordination of reflex components, that is to say, the reflex in which both limbs move in response to stimulation of one limb only. Since he observes this pattern of reaction, as just noted, in fetuses of from 5 to 5.5 cm., he therefore must regard that co-ordination of movement occurs at that early age.

That there is utter lack of co-ordination in these early movements of fetuses, or that the movements that are called diffuse by Minkowski and Bersot are simply an aggregation of physiologically disconnected elements does not seem possible when the evidence is assembled from all available sources.

Minkowski himself describes the anal reflex of a 13 cm. fetus as involving with contraction of the musculus constrictor ani strong simultaneous movements of both legs backward. This reaction, particularly the simultaneous reaction of the legs, certainly expresses co-ordination of movements, and this is far within the age limits of the group of fetuses the movements of which he says are inco-ordinated. He describes also leg movements that occur simultaneously with movements of the mouth in fetuses of 3.5 and 6.5 cm. In the same two specimens both legs participated in what Minkowski designates as the eyelid reflex. The grasping movement of the toes which he describes for a fetus of 6.5 cm. must also express co-ordination. In a fetus of 10 cm., Minkowski says definitely, the shoulders moved at the same time with flexure of the trunk, neck and movements of the limbs. The simultaneity of action in all these components of a given performance must mean co-ordination. Minkowski reports also that he has seen in a fetus of 13.5 cm. the touch of a hand induce, besides flexion of the two arms, repeated movement of the mouth and simultaneous drawing of the head backward. Again, the simultaneity of action, which is explicitly described, must mean co-ordination. It seems to me, therefore, that Minkowski's particular descriptions of individual cases contradict his general statement previously cited that the movements of human fetuses of lengths from 5 to 23 cm., or from the ages of 2 to 5 months, lack co-ordination.

Bearing on this question also is the observation of a fetus of 5.5 cm. by Bolaffio and Artom. In this case, the authors say that a tapping with the finger lightly on the table on which the specimen lay in a dish elicited repeatedly strong symmetrical movement that involved elevation of the shoulder, abduction of the arm and flexion of the thigh and leg. These movements, they say, could be elicited at will for about three minutes. According to the authors' description, this fetus of 5.5 cm. exhibited consistently a definite behavior pattern that must, it seems to me, be interpreted as expressing co-ordination. I am led, therefore, to the conclusion that the term "diffuse" as used by Minkowski and Bersot cannot mean lack of co-ordination or integration.

THE SIMPLE REFLEX IN PHYSIOLOGY

Sherrington's conception of the reflex as a nervous function is well known, but it should be stated here in order to make a comparison of the purely physiologic with the embryologic interpretation, as presented in this communication, definite and clear. "The unit reaction in nervous integration," Sherrington says, "is the reflex." The "simple reflex" is that grade of co-ordination which makes "an effector organ responsive to excitement of a receptor, all other parts of the organism being supposed indifferent to and indifferent for that reaction." "In this simple reflex," he adds, "there is exhibited the first grade of co-ordination." Nevertheless, he says explicitly, "a simple reflex is probably a pure abstract conception, because all parts of the nervous system are connected together and no part of it is probably ever capable of reaction without affecting and being affected by various other parts, and it is a system certainly never absolutely at rest." But nowhere in the conventional physiology of reflexes do I find the unity of the reflex with the total behavior pattern revealed in such clearness as I see it in the development of reflexes in *Amblystoma*. That these reflexes are in the same category with those of neurophysiology I have explained elsewhere. In development, the simple reflex proves to be what Sherrington suspected it to be, "a pure abstract conception"; and while Sherrington regards it as necessarily affected by and affecting "various other parts" of the nervous system, in development it appears as inextricably fused with the total behavior pattern. This feature of it I have treated more at length in my lectures on "Anatomy and the Problem of Behaviour." There I have shown how in its very beginning the exteroceptive reflex is conditioned by proprioceptive or postural conditions of the animal as a whole. From this total pattern the reflex emerges more or less in discreteness, but in the light of the knowledge of its development, to hold that it ever acquires independence from the total pattern so as to constitute a "unit reaction" is begging the question; and to found the physiology of the nervous system on an erroneous abstract conception is, to say the least, not working along the most direct line of progress. Neurophysiologists obviously appreciate that the "simple reflex" is not actually a simple affair, but unfortunately it is accepted by many writers on psychology as a real unit of structure and function, and larger patterns are regarded by them as arising by an integration of these units. Nevertheless, all the evidence that has accumulated from the study of the development of reflexes, and particularly from the studies by Bersot, definitely establishes that the reflex is in no real sense a unit.

Bersot has made extensive statistical studies of the plantar reflex in development from fetal into postnatal life. He says that the external stimulation acts, not on one determined part of the organism, but on the general organic condition, which is variable; that the organism is interposed between the stimulation and the reaction; the different modes of reaction are not in immediate connection with each other, but in a connection determined by the general condition. This last statement by Bersot is corroborated by my observations on the development of the walking gait in *Amblystoma,* where, as already noted, the reaction of one limb is not in immediate relation to the action of any one of the others, but the action of each limb is in direct relation to the trunk as a whole, and the action of the latter is the agency of integration of the limbs.

Elsewhere, Bersot says that if one judges only by the motor reaction, after the plantar reflex has become differentiated, one will conclude that the stimulus which formerly acted on the entire organism now acts on the lower limb alone, but, he adds that is not so: the stimulus still continues to act on the entire organism. Again he asserts that the plantar reflex of the infant shows one clearly the impossibility of considering reactions as varying independently one of another, and that each manifestation, the whole of which constitutes the plantar reflex, is then to be considered as an integral part of a whole which evolves in proportion as the organism develops. These conclusions of Bersot from his extensive and highly technical statistical studies of the development of the reflex are exactly in accord with the development of reflexes in *Amblystoma* as I have seen them, and they certainly refute the idea that the reflex as seen in physiology has that fixed individuality which characterizes a unit. They are also corroborated by the observations by Minkowski, who endorses them, and asserts that in a certain sense and at least in latent form the whole organism participates in every reaction, so that what impresses one as a reflex constitutes only the especially manifest and visible form of it.

This interpretation of dominance of the organism as a whole over reflexes is further supported by the early development of postural reflexes in the human fetus as demonstrated by Minkowski, and by the observation by Bolaffio and Artom of shoulder movements repeatedly in a human fetus of 7 cm. in response to the prick of a pin in the cerebral cortex, and an increase in the strength of reflexes after ablation of the cerebral hemispheres in this same specimen. According to the latter observation, the cerebral cortex begins to participate in the behavior of man approximately with the beginnings of the oral reflex, the eyelid reflex and the grasping reflex of the toes, and while virtually a total reaction can still be stimulated by a touch on the arm or body.[3]

It appears, therefore, that from the beginning of the process of development of reflex specificity of receptors and the individuation of reflex patterns, the cerebral cortex of man operates with the rest of the nervous system as a unit. Accordingly, if there is such a thing as a "unit reaction" in nervous function, it is the total pattern; and the development of specific nervous functions, such as reflexes of different grades, is an analytic process, not a synthetic one. This being true, any impairment of the integrity of the nervous mechanism as a whole, either experimentally or pathologically, must necessarily accentuate the analytic process, and bring subsidiary mechanisms out into a relation of relative dominance over a fragment of the system. The study of such subsidiary mechanisms has added incalculably to the knowledge of nervous functions; but their actual meaning, it seems to me, cannot be adequately revealed without exhaustive knowledge of their origin and development in the growing organism.

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[3] In the discussion of their observations Bolaffio and Artom seem to disclaim any evidence of cortical function during the first six months of fetal life. They say that the cortex (rolandic and other zones) and the pyramidal tract in the internal capsule and in the peduncles are unexcitable during this period. In this conclusion they are no doubt in accord with the prevailing opinion. Nevertheless, they say, also, that after decerebration they observe constantly an increase of the muscular reflexes, and they cite explicitly their cases number 2, of 7 cm.; number 5, of 23 cm.; number 9, of 33 cm.; and number 11, of 34 cm. So far as their evidence goes, it appears that decerebration has an effect on the behavior of fetuses of 7 cm. and older, and that, although the cerebral cortex may not be excitable to mechanical or electrical stimulation at this time, it must participate in the function of the nervous system as a whole.

In conclusion, I am convinced, by a study of all available records of movement in human fetuses of the first 6 months, that behavior develops in man as it does in *Amblystoma* by the expansion of a total pattern that is integrated as a whole from the beginning and by individuation of partial patterns (reflexes) within the unitary whole. This principle of embryologic development of the function of the nervous system seems to me to have important bearing on fundamental problems of both normal and pathologic physiology and psychology.

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GLOSSARY

Abduction, movement away from the middle of the body (median line).

Adduction, movement toward the middle of the body (median line).

Amblystoma, common salamander.

Amphibian, water and land animal; a class of vertebrates; the frog and salamander are examples.

Anterior, headward; front; forward.

Antigravity Component, portion (of a movement) that occurs against gravity.

Aponeurosis, attachment of a muscle; fibrous extension of a muscle.

Arrhythmic, not rhythmic.

Asymmetrical, not symmetrical.

Axial Musculature, muscles along the main axis of the body.

Axial Organization, organization of growth processes with respect to the axes of the body.

Babinski Reflex, extension of the toes elicited by stimulation of the sole of the foot.

Bilateral, pertaining to both sides (of the body).

Chain Reflex, series of discrete reflexes "chained together" by one reflex acting as the stimulus for the next.

Constrictor Ani, muscle closing the anus (opening of the rectum).

Distal Spinal Centers, centers at lower end of the spinal cord; Distal, away from the center.

Dorsal Flexion, bending backward.

Dorsum Surface, back surface.

Electrolyte, a solution that conducts electricity.

Extension, a straightening out of a flexed limb.

Exteroceptive, pertaining to sense organs stimulated from external sources, sense organs of the eye, ear and skin.

Fetal, pertaining to advanced stage in the embryonic growth of mammals.

Flexion, a bending, as in closing the fingers against the palm.

Homolateral, on the same side of.

In Utero, before birth; while in the uterus.

Metamorphosis, transformation of structure in a life cycle.

Neuroblast, undeveloped, embryonic stage of a nerve cell.

Orbicularis Palpebræ (Oculi), muscle that closes the eyelids.

Patellar Reflex, reflex elicited by striking just below the knee-cap (patella).

Pectoral, pertaining to the chest.

Percussion, blow; tap.

PLANTAR REFLEX, contraction of the toes elicited by stimulating the sole of the foot; PLANTAR, pertaining to the sole of the foot.

PROPRIOCEPTIVE, pertaining to the sense organs in the muscles, stimulated through muscular contraction.

PROXIMAL PHALANGES, parts of the fingers (bones) nearest the wrist.

THORAX, chest.

TRIGEMINAL NUCLEI, nerve centers in the base of the brain belonging to the trigeminal nerve, a facial, sensory nerve.

UNILATERAL, all on the same side.

Urodela, subdivision of amphibians.

READING TWENTY-EIGHT[1]

BIOLOGICAL FOUNDATIONS OF SOCIAL INTEGRATION[2]

Editor's Note

The following article by Professor Child is a popularization of his gradient theory of physiological integration. He is drawing a parallel between integration as seen in physiology and integration as seen in sociology. In other words, he is pointing out his belief that the same organismic laws that hold in biology, hold also in a study of social behavior. Much of his language is figurative for the reason that he was explaining the facts of physiology to an audience not trained in that subject. The general character of his discussion has already been indicated in the note to the preceding Reading.

The last Reading brings the student back, in a sense, to the first one. Throughout the study of behavior he has been examining facts varying all the way from the crude to the precise, the general to the specialized, the social (through the psychological) to the physiological. Yet all of these facts can be organized with respect to the same set of principles. But he should remember that there is nothing final about a theory. Its purpose is not that of furnishing an ultimate explanation of anything, rather it is that of systematizing a set of facts with the view of understanding them in their relationships.

Theories are indispensable to the search for new problems and to the better understanding of old problems. They are indispensable to the practical application of scientific facts, for unless the facts are intelligently organized and interpreted, they have no practical value. New applications follow advances in theory, but the student should remember that a theory is advanced in an effort to make isolated facts intelligible and to lead to new facts; it is a working hypothesis, designed in a search for truth, but an hypothesis that must always give way to another when the facts demand it.

ABSTRACT

Biological foundations of social integration. The fact that the organism behaves as an orderly whole suggests the existence of integrating and ordering factors of some sort. Physiological investigations of recent years show that in all except the simplest

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[1] Parallels *The Science of Psychology*, Chapter XVII.

[2] By C. M. Child, University of Chicago. From the *American Journal of Sociology*, Vol. 22, 1928, 26-42. By permission.

organisms, which apparently represent merely a surface-interior pattern directly dependent upon environment, physiological integration results from the establishment of a physiological dominance, a leadership or government in the protoplasm composing the organism. Experiment demonstrates that this dominance constitutes a real ordering and integrating factor, an actual government. The primitive dominance represents an autocracy or a ruling class, but with the progress of individual development and of evolution, physiological integration becomes increasingly complex. Secondary governments arise in various parts, all subordinate to the general government, and the government becomes increasingly representative; that is, the subordinate parts are increasingly able to influence the dominant regions. In the higher animals and man we find an almost inconceivably complex and highly efficient mechanism of physiological government which is representative to a high degree. To the biologist social integrations examined in the light of the physiological processes appear, not as something new and unique, but as the further development and complication of the behavior of living protoplasms, of which organisms are the most general expressions. The foundations of social integration seem to him to lie, not in the simplest social processes in man or in the animals, but in the ability of living protoplasms to react to their environment, to transmit the effects of such reaction, and to keep some record of past reactions which influence the character of later behavior.

Before proceeding to consideration of my subject let me make it clear that I speak as a biologist. It would be an unwarranted venture for me to attempt to do otherwise. As I understand it, the invitation to take part in the program of this society came to me simply because my field of investigation has had to do with some of the problems concerned in the integration of living units into orderly wholes, and because the physiological processes of integration have seemed to me to be suggestive in their relations to the processes of integration among human beings. I shall attempt to tell you something of what the biologist has learned or thinks he has learned about the physiological processes of integration which make organisms orderly wholes, and to indicate very briefly how certain aspects of social integration appear to him in the light of what he has learned in his biological investigations. Social integrations represent the highest development of integrative processes among living units, and a consideration of the question whether they have any relation to the simpler physiological integrative processes may be of some interest to the sociologist. It will be convenient at times to use sociological terms for biological phenomena; in fact, it is sometimes difficult to avoid using them; but I shall attempt to restrict their use to those cases in which it seems to be justified biologically, whatever the sociologist may think about it.

Physiological correlation.—In the rather recent past biology was so largely concerned with learning about the structure of living things and the functions of their different organs that it gave little attention to the problem of the unity and order which makes the organism what it is. The unity and order were ignored, taken for granted, or sometimes translated into some sort of pre-established harmony. Some biologists wrote of the "organism as a whole," or of the influence of the whole upon its parts without knowing very clearly what they meant, beyond the fact that a wholeness of some sort was evident, and that a unity and order, a harmony of some sort, existed among the parts. With the development of the experimental method, however, it became possible to throw some light on the nature of this order and wholeness. In the simpler organisms it is possible to separate parts from each other without killing or seriously injuring them, and so to determine their reactions when separated. It is also possible to put parts together in arrangements different from those in the ordinary individual and to determine how they behave under such conditions. And since the susceptibility of different regions and organs to external agents differs, it is possible to inhibit the activity of certain parts without much affecting others, and to observe the changes which follow. On the other hand, we can stimulate certain parts and observe results and we can isolate certain substances produced by particular organs, for example, thyroidin, adrenalin, etc., and determine their action. These and various other lines of investigation have established the fundamental importance of what is commonly called physiological correlation for the unity and wholeness of the organism. Physiological correlation consists in the physiological influences and actions of parts on each other, in short, of all the physiological relations between parts.

The factors in physiological correlation fall into two great groups: the transmissive and the transportative relations. The transmissive relations consist in the transmission of energy changes from one part to another. For example, mechanical effects are transmitted in every movement of the body. Transmission of heat, of chemical and electric energy also occurs, but more important than any of these is the transmission of the complex physicochemical process called excitation. All nervous control, all mental activity, is associated with such transmission. This transmissive group of correlative factors represents what we may call the processes of communication between parts. Transportative correlation consists in the transport in mass of substances from the part producing them to other parts. These transportative factors represent the barter and exchange, the commercial relations between the various parts. The organism is then not a mosaic of independent parts, but consists of the parts plus all the relations between them, and the wholeness, the unity consists rather in the relations than in the parts themselves. It is not enough to demonstrate the existence of these physiological relations between parts. We must investigate their origin and the changes which they undergo in the course of individual development and of evolution.

The simplest organisms.—The simplest organisms, certain bacteria and some other forms, appear to have no definite parts other than the surface and the interior. Because of its exposure to an external medium, the surface differs from the interior. Since the surface is the medium of relation and exchange with the external world, it exercises a certain control over what happens in the interior. But we find that surface and interior in such organisms are interchangeable, that is, the interior is capable of transforming into surface if it is exposed to the external medium, and parts of the surface buried in the interior soon become like other internal parts. In other words, the pattern of such organisms is directly dependent on relation to environment, and changes as the relation changes. Evidently then the integration of surface and interior into an orderly whole does not occur autonomously in the protoplasms of such organisms, but results from the reaction of the protoplasm to its environment. The pattern of such an organism is a behavior pattern: it represents in each case the behavior of a particular kind of protoplasm under certain environmental conditions.

Axiate organisms.—All organisms except the simplest show, in addition to this surface-interior pattern, another integrative pattern which we call axiate. that is, they possess physiological polarity and symmetry. Polarity and symmetry are expressions in the mass of protoplasm or cells which constitute the organism of orderly arrangements and relations which are referable to certain so-called axes passing in certain directions in the body. The polar axis is the chief or major axis, that is, the direction along which the primary or major order arises. Ordinarily it is the longitudinal axis, the anteroposterior direction in the body. The axes of symmetry represent secondary orders in other directions.

Since polarity and some sort of symmetry are fundamental features of the structure and the functional relations of parts of the organism, the problem of their nature has engaged the attention of many biologists. In the past they have often been regarded as inherent in the molecular structure of the different protoplasms, and as perhaps analogous to the polarity and symmetry of crystals. The experimental investigation of recent years not only affords no support to this view, but indicates that polarity and symmetry are not expressions of static molecular structure in the substratum of life, but rather of the dynamics of life itself. All the evidence indicates that polarity and symmetry in their simplest terms consist in a quantitative differential in physiological activity and in protoplasmic condition associated with it, that is, the processes of living, or certain of them, are going on most rapidly at one end of such an axis, and from this end their rate decreases along the axis. These differences in rate of living at different levels determine the formation of different organs. For example, the head, and primarily the brain, develops from the most active region of the polar axis and the regions of the central nervous system posterior to the head from the most active regions of the axis of symmetry. Other organs arise in definite order along the axes. The order and arrangement of organs depend on the quantitative differences along the axes, but the kinds of organs which develop in a particular case depend upon the hereditary constitution of the protoplasm concerned.

Moreover, many different lines of evidence indicate that such a differential or gradient does not arise autonomously in the protoplasm, but rather as a reaction of the protoplasm to a differential in its environment. In the protoplasms of many of the simpler animals it is possible on the one hand to obliterate previously existing polarity or symmetry by decreasing the differences in activity at different levels of the axis and, on the other hand, to determine new axes of polarity and symmetry by establishing new regions of high activity through the action of external energies on the protoplasm. In this way we can obliterate previously existing axiate patterns and determine new ones in other directions and literally make the individual over. So far as our observations go, it appears that in nature also polarity and symmetry do not arise autonomously in protoplasms, but originate in reaction to environmental differentials, for example, in the differential relations of the different regions of the egg to the parent body or to other environmental factors.

If these conclusions are correct, the patterns of living things, that is, the structural and functional orders and relations, originate in the last analysis in the reactions of protoplasms to environment. They are, in short, behavior patterns. According to this view organisms represent the most general and fundamental behavior-patterns of protoplasms, and these patterns become the foundations of the more specialized forms of behavior. The establishment of a new persistent axis of polarity or symmetry in a cell or a cell mass represents a record in the protoplasm of reaction to environment. This record of past behavior becomes a fundamental factor in determining the character of all later behavior and may even persist through cell division and various reproductive processes and so become hereditary, but such inheritance is obviously not Lamarckian inheritance of acquired characters. Is not such a record of past reaction, together with its effect on all later behavior, the most general biological form of learning by experience?

Physiological control or dominance.—Granting that organismic pattern originates as a behavior pattern, a record of experience, the question at once arises, How does a quantitative differential in physiological condition bring about integration, How does it determine the wholeness of the organism? We find that when we establish a region of high activity in a cell or a cell mass by the local action of external energies such a region influences adjoining regions within a certain distance, that is, the activity locally induced does not remain sharply localized, but spreads to adjoining regions, the effect decreasing with increasing distance from the point of origin. The active region then becomes the dominant factor in determining a gradient in activity. It exercises a control over regions within a certain distance from it because it is the chief factor in determining the degree of their activity. This influence of an active region on adjoining regions is apparently due primarily to the transmission of energy changes from it and it is probable that electric currents resulting from the differences in condition between it and the adjoining regions are factors in such transmission. This relation between a more active and a less active region appears to be the foundation of physiological dominance or control. Such dominance and subordination of parts in one form or another is a characteristic feature of physiological axes. Experiment shows that it is an essential factor in determining the positions and proportions of parts along the axis. It represents the first step in physiological integration beyond the surface-interior pattern. From it develops in plants the control which the growing tip exercises over other parts in preventing their development into new growing tips like itself or in determining their growth in certain ways as subordinate parts, *e.g.*, as lateral branches. That the growing tip does actually control in such cases is shown by the changes which occur when the tip is removed or inhibited. Buds which were previously inhibited now grow out, lateral branches transform into main stems, etc. The growing tip of the plant represents the most active region of the axis and its dominance depends on its activity. In animals the head, that is, the region of the brain and the chief sense organs, arises from the most active region of the polar gradient, and out of the primitive dominance of the active region in the earlier stages develops nervous control.

In its more primitive protoplasmic forms and in the nervous system physiological control depends on the transmission of energy changes rather than on the transportation of substance, that is, it represents communication rather than commerce between the parts concerned. But as the different organs become increasingly different in constitution the chemical reactions in them become different in character and give rise to different substances. These differences which take place both in individual development and in evolution provide a basis for transportative relations between various parts. A product of the activity of one may be transported to, and influence the activity of, another. In short, with the progress of differentiation commercial relations become possible within the organism and increase in variety and complexity. Such relations, however, are not fundamental factors in physiological integration, for they are not possible until a certain degree of integration and differentiation is present.

The dominant region represents a real physiological leadership, a government. It is the pacemaker in development and the primary factor in physiological integration wherever it persists long enough to be effective. The reality of this leadership cannot be too strongly emphasized. This conception of dominance is in no sense speculative, but represents a physiological fact established by many different lines of experimental evidence. By means of experimental methods we can depose existing leaders and establish new ones with effects on the organism similar to those which occur in any integration dependent on leadership or government.

According to this conception of the organism, the unity and order are not pre-established in the protoplasmic constitution, but develop out of protoplasmic behavior. Only the simplest sort of integration is possible without definite and more or less persistent dominance, that is, leadership. In the surface-interior organism where there is no such leadership differentiation is of the same simple and primitive character as in a herd of animals surrounded by other animals that seek to attack, or an old-time wagon train surrounded by Indians. In organisms of this sort temporary leadership sometimes develops, *e.g.*, an amœba may send out a pseudopod and begin to move in one direction, and in the herd or the wagon train something may start a break in a certain direction and an axiate pattern arises temporarily.

In the axiate organism the dominance or leadership of the most active region becomes more definite and more permanent. This dominance initiates an orderly course of development and differentiation, the relations of different regions to the dominant region determining what part each shall play in the organism. In many of the lower organisms the persistence even of the structural pattern depends throughout life on the control exerted by the dominant region. In such forms it is possible to demonstrate, by cutting the animal into pieces, that the parts possess other capacities than those which have been realized in their development as parts. For example, every level of the body may be capable of forming a head, but as long as it is subordinated to the dominant region, the head, it has no opportunity to develop as a head, but is forced to develop into something else. The capacity for leadership is there, but the opportunity is lacking as long as the original leadership persists. In many other forms this is true only in the earlier stages of development. In the more advanced stages the structural pattern becomes relatively fixed and physiological control is concerned primarily with function of the various parts.

In many of the simpler organisms, and particularly in the earlier stages of development, dominance may be limited in range by the primitive character of the processes of transmission, that is, with primitive means of communication, physiological leadership is effective only over a relatively short distance. This means that the size of the mass which can be integrated into an individual is also limited. The limit of integration cannot exceed the range of effective communication. This limitation of the range of dominance makes possible what we call physiological isolation of parts. For example, if growth in length continues beyond a certain limit the body becomes too long for complete control by the dominant region, and the part farthest away from that region loses its characteristics as a part and begins to develop into a new individual. That is, it develops a new leadership either from that part of the mechanism of government already present or directly from its own relations to environment.

If, on the other hand, the activity of the dominant region, that is, the effectiveness of the leadership, the government, decreases, the range and effectiveness of control decrease and parts of the individual may become sufficiently isolated, even without growth, to give rise to new individuals. If the original dominance disappears completely, the organism may separate into its constituent cells, and each of these may begin a new course of development if it is able to develop a dominant region from the part of the original mechanism of control which it contains or if it acquires a new dominant region through its relations to environment. If the dominant region is removed, its functions may be taken over by the parts most closely associated with it, but before they are able to attain full control isolation of some part and reproduction of a new individual from it may begin. In social integrations of various sorts, particularly among the more primitive, similar situations arise. The personal leader may be weakened by age or disease or may die, or institutional dominance may change from an actively integrating force to a mere formal control. It is a familiar fact that such conditions favor the appearance of new groups from parts of the old, or the complete disintegration of the group into its constituent units.

In the organism we can block the process of control on its way from the dominant to the subordinate region; we can, so to speak, cut the wires. As a matter of fact, we do not need to cut them, for it is possible in certain cases to block physiological control by merely cooling a short portion of its path and so decreasing the activity of the protoplasm there to such an extent that it does not pass on the message. This procedure also isolates physiologically the subordinate part concerned and may bring about the development of a new individual from it. I scarcely need point out that isolation of parts in this way is the physiological prototype of certain social processes of reproduction. The blocking in one way or another of communications from the leader or the government may bring about reorganization through revolt or otherwise.

And finally, by increasing the activity of a subordinate part it is often possible to develop within it a new leadership which is effective in determining a new integration in spite of the original dominance. In plants, for example, buds which happen to be exposed to particularly favorable external conditions may develop as new axes in spite of the dominance of the growing tip. In other words, environment favors the development of local government in such a part, and we see similar situations arising in social groups.

Cancer consists of cells of the body which have undergone change in some way not yet understood so that they do not react to physiological control. Such cells have become complete anarchists. They live as individual cells and do not integrate into orderly wholes. They are extremely active cells, and have for some reason run wild. It is of interest to note that conditions are particularly favorable for the appearance of such cells in the old individual in which physiological control is not as effective as in the young. Investigation concerning the cancer problem is directed primarily toward discovering first what makes these cells run wild, and second, how to get rid of them or make them behave normally again, after they have run wild. Investigations into certain political situations have objectives very similar to those of cancer research, though the method of procedure may differ widely.

This account of the processes of physiological isolation is not speculative, but a statement of physiological fact. The various processes of physiological isolation of parts determine the forms of asexual reproduction, budding, fission, etc., which occur in the plants and the simpler animals, and physiological isolation and reproduction can be induced experimentally by all four of the methods described previously. The art of pruning and trimming plants is based entirely on the relations of leadership or dominance and subordination, and many of the lower animals which resemble plants in their branching form can be similarly pruned and trimmed with essentially similar results.

The development of integration.—The axiate organism, at least in its simpler forms, begins as an autocracy, or, if we speak in terms of individual cells, the multicellular organism usually represents government by a ruling class. I mean by this statement that the dominant region determines the behavior of other parts, but is itself independent of them, at least as regards the development of its general structural pattern. The dominant region in such forms develops in advance of other parts and is able to develop even though other parts are entirely absent. For example, small pieces of such organisms may develop into nothing but heads. They are leaders with nothing to lead. In many of the lower forms we find by cutting the body into pieces that every level is capable of developing into a head when it is isolated from the parts anterior to it. In such cases every level has the capacity for leadership, but under the usual conditions only one level has the opportunity to become the actual leader.

In the simpler organisms and in early stages of development the control is usually limited in range because of the primitive character of the means of communication, but with the development of special organs of communication, *e.g.*, the conducting tissues in plants, the nervous system in animals, the range of control and the size of individual which can be integrated increase greatly and become indefinite, that is, the nerves can conduct impulses to indefinite distances and the actual size is limited by other factors. With this improvement in the means of communication in the course of individual development and of evolution, the primitive autocratic character of leadership in the organism undergoes change. The range of dominance is not only increased, but it also becomes possible for the subordinate parts to communicate with the dominant region and to influence its behavior. The head is no longer independent of other parts, but the further development of its structure and its function is determined in part by the other organs of the body.

With still further advance in development and evolution integration becomes still more complex. Leaders of various organs arise, but are still subordinate to the leader in chief, the brain. There is in the heart, for example, a leader, a pacemaker, which determines the rhythm of the beat. This pacemaker arises from the most active region of the embryonic heart. If we remove it or inhibit its activity another region becomes a pacemaker, but is not, at least when it first becomes the leader, as effective as the original leader. We find similar pacemakers controlling the muscular activities of the alimentary tract and other axiate organs of the body.

In the higher animals there develops in the central nervous system an almost inconceivably complex mechanism of communication and control with a hierarchy of centers, or bureaus, each with a limited dominance and subordinate to those of higher rank, and all subordinate to the cerebral cortex. Physiological integration in the higher vertebrates can be compared only to a highly developed representative government with an elaborate system of bureaus for attending to the needs and demands of the various parts of the body.

Although we know little as yet concerning the differences in different nerve impulses, we can see that different nervous communications are handled differently in the central nervous system. For example, a local stimulation, a tickling of the skin or a slight prick, gives rise to an impulse which goes into the spinal cord and is handled there as a routine matter by one of the subordinate bureaus and the order is sent out for the contraction of certain muscles to remove the part from the source of irritation. This is what we call a reflex, and does not involve consciousness. A more important or more urgent communication from some organ may be passed on to a higher bureau and the return communication may have reference not merely to this particular communication but to others which have reached the bureau at about the same time or which have left records in the nervous structure to serve as precedents. If the communication is sufficiently important, or if the action of the lower bureaus does not meet the situation, it may be relayed to successively higher bureaus and may finally reach the cerebral cortex. Even when the communication is handled by a lower bureau, information concerning it is often sent to the cortex. The cortex has often been likened by neurologists to a parliamentary assembly combining the functions of deliberation, legislation, and the judiciary. The communications from the cortex have reference, not only to the state of the body as a whole at the moment, but to the records of past states. That is to say, their character is influenced, not only by the communications which are coming in at the time, but by the records in the nervous system of past communications. In short, the nervous system of man and the higher animals represents an almost inconceivably complex mechanism of government, representative to a high degree and remarkably efficient. The organism as a whole is comparable to a great nation-state, but with relatively rigid class distinctions. Between the different classes complex commercial relations have also arisen in the form of transport of nutrition, oxygen, and waste products, of internal secretions, so-called hormones, etc., and these also play a part in the further differentiation of the various organs. Such organisms have progressed a long way towards democracy from the primitive autocracy or government by a ruling class.

In these higher forms of physiological integration the size of the individual is not limited by the means of communication, as it often is in the lower forms, for in the higher forms communication is possible over indefinite distances. In man and higher animals size is limited by the decrease in capacity for growth with the progress of differentiation. In a young developing organism the additions to body substance exceed the losses through breakdown; the protoplasmic birth-rate exceeds the death-rate; but as the organism ages its metabolic activity decreases and it merely maintains its size instead of growing, and finally in extreme old age the protoplasmic death-rate may exceed the birth-rate. In the lower forms parts which are physiologically or physically isolated and reorganize into new individuals undergo more or less rejuvenescence. Their metabolic activity increases; they resume growth; and the new individual formed is in all respects younger than the organism of which it originally formed a part. Increased vigor in consequence of social reorganization is a common phenomenon.

Physiological integration of higher orders.—In the plants and in many of the simpler animals individuals arising as buds in consequence of partial escape from the control of the original dominant region do not entirely separate, but form a complex of individuals. The stems and branches of plants and the branching plantlike forms of certain animals are complexes of this sort. In these complexes of individuals various degrees of integration similar in character to those already considered appear. In some such complexes the component individuals are all alike and the whole is a mere aggregation of individuals without dominance or appreciable integration, except perhaps temporarily. Very commonly, however, dominance and subordination among the component individuals develop, and the whole, or various parts of it, may be integrated into an individuality of higher order than that of the members which compose it. In some cases the original individual gives rise to the others by budding and retains its leadership throughout the life of the complex; in others each new individual becomes dominant for a time and then gives place to another. The relations between the component individuals of such a complex are concerned in determining its growth form and other aspects of its behavior. Among the plants, for example, the fir and the spruce and related forms are cases of continued dominance of a single leader, the growing tip of the main stem. This growing tip determines the growth of all buds as lateral branches which are more or less bilaterally symmetrical while the branches are radially arranged on the main stem. If the growing tip of the main stem be cut off or inhibited the uppermost lateral branches or some of them change their form of growth and become new main stems and assume dominance over the whole. In such trees as the elm, on the other hand, various leaders of approximately equal influence arise sooner or later and a spreading crown consisting of more or less similar axes results. Physiological integrations of this spreading sort are better adapted to a sessile life rather than to an actively motile habit and have developed most extensively among plants, but various sessile animals, for example the corals and some of their relatives, as well as some other animal forms, have developed complexes of this kind. For actively motile organisms which progress through their environment instead of spreading out in it a more compact and efficient integration than this is necessary.

Social integration among animals.—Various degrees of truly social integration, or at least what the biologist regards as social integration, occur among animals, and some of them differ from the physiological integrations chiefly in that they take place between components which are not in protoplasmic continuity. They range from mere aggregations without integrating factors other than the common reaction of all members to some environmental factor to groups with various degrees of leadership or control. Herbs of various species often assume a more or less definite surface-interior pattern in the presence of their enemies.

In other cases real leaders exist and more or less definite and persistent integrations occur.

In connection with integrations of this character, whether in animals or in man, the question of the significance of sex and the family arises. The family appears, at least as a temporary or periodic integration, rather widely among animals, and in some cases is surprisingly persistent. From the biological standpoint, however, the family does not seem to have the importance for integration which has been assigned to it by some sociologists. The sex relation and the relation of parent and offspring afford a biological basis for limited association of individuals, and it is evident that because of their general occurrence in some form or other these relations may play a part in integrations which may equally well arise independently of them. Various social integrations may develop out of the family, not because it is a necessary or fundamental factor in their origin, but merely because it antedates them. Undoubtedly the family has played an important part in the history of social integration, but in what we may call the physiology of social integration it seems to the biologist to possess only a limited significance. Among animals, as among men, some social integrations develop in relation to sex and the family while others are obviously independent of them. Some animal species in which the family is well developed show little or no other social integration, while others in which the family scarcely appears develop definite social groups.

The social insects, particularly the termites, bees, and ants, seem to represent social integrations developing, at least in part, on the basis of sex and the family. These societies are matriarchal, but the leadership is apparently in no sense personal. The queen mother has become an institution around which the society centers. Wheeler has suggested that social life among the insects originated in the exchange of nutritive substances including secretions and excretions, but it seems more probable that much of this exchange has arisen secondarily in the development and complication of family relations. I am inclined to believe also that the lack of plasticity and the complete subordination of the individual to the society which is characteristic of the most highly developed insect societies are in some measure due to their development on the basis of sex and the family. Integrations developing on this basis must, it seems to me, be relatively rigid and unplastic because the possibilities are rather narrowly limited. In the most highly developed ant societies these possibilities of integration on a familial basis seem to be more completely realized than in any other case.

Conclusion.—In this brief consideration of integration I have endeavored to show that the organism represents, first of all, a behavior pattern in the cell or cell mass of which it consists. Physiological integration is not an autonomous process, but originates through relation and reaction to environment. The likeness of different individuals of the same species apparently results, first from the fact that their protoplasms are similar though not identical as regards hereditary constitution and potentialities, and second from the fact that environment has been to a considerable degree standardized for each species in the course of evolution. If we alter the environment beyond the normal range we alter the course of integration and the resulting individual. In short, the organism appears to be the expression of an integrating and ordering institution which originates in the most general relations and reactions of living protoplasms to environment. The organism is inconceivable except in relation to environment, and that means that it is inconceivable except in terms of behavior. Integration is not the mere aggregation of units, but rather the development of definite relations between them. The relations, not the parts, are the real integrating factors. The organism is not merely the sum of its cells or organs, but of these plus all the physiological relations between them, and to the biologist the same appears to be true of human society. A cell as a part of an organism behaves differently and is therefore different in some way from that cell isolated. The biologist finds it difficult to believe that the individual human being as a member of a social group is the same as that human being isolated from the group.

In both the organism and society the primary factors in integration appear to be the transmissive rather than the transportative relations, that is, communication rather than material or commercial exchange. Communication does not require special organs of transmission, but may take place through various physical media, and whether it occurs through living protoplasm or some other medium is unimportant. Apparently all that is necessary for the beginning of orderly integration in protoplasm is a quantitative difference in rate of living and the possibility of communication. Dominance or leadership in its most general physiological form apparently originates in the more rapid liberation of energy. The personal leader among men may not always have a higher metabolism and liberate energy more rapidly than those he leads, but in consequence either of his relation to the particular situation or his heredity or past experience he reacts to the situation in such a way as to develop an integrating force, usually psychological rather than physical. In social development we see leadership becoming impersonal and institutional, but in all cases the dominant conception is the one most effective in determining integrative behavior of different individuals with reference to a particular situation. To the biologist social integration appears first of all as behavior of living protoplasms, and the evidence at hand seems to indicate that from the behavior which integrates the individual organism into an orderly and definite whole to that which integrates a nation, a church, or other social group there is essential continuity. The foundations of social integration seem to him to lie, not in the simplest social integrations among men, not in the social integrations among animals, but in the ability of living protoplasms to react to environment, to transmit the effects of such reactions, and to maintain records of past reaction which influence present and future behavior. In human society new forms of dominance appear, new means of communication are developed; but in spite of such differences the fundamental principles and laws of social and physiological integration appear to be essentially similar in that they both represent general behavior patterns resulting from the reactions of living protoplasms to their environments.

GLOSSARY

Amœba, one-cell water animal.
Lamarckian Inheritance, transmittance of traits acquired through use or disuse.
Learning by Experience, learning with experience.
Matriarchal, dominated by a mother or queen.
Polar Axis, axis exhibiting polarity.
Polarity, organization with reference to ends, poles, stresses.
Protoplasm, living substance or tissue.
Pseudopod, temporary "foot" or "leg" thrust out by amœba in process of locomotion; temporary extension of the protoplasm.
Thyroidin, secretion from the thyroid gland (in the neck).

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